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Extrasensory Perception

Chicanery in the spiritistic field has long been recognized and long been a bane to those who attempt to examine under controlled conditions the reality of the alleged powers of telepathy and clairvoyance. Among recent workers in this field, S. G. Soal of London and J. B. Rhine of Duke University have been the recognized leaders. They and their colleagues have conducted thousands and thousands of trials, with many subjects, under varied conditions, and have produced such a substantial body of data as to convince many a once skeptical critic that extrasensory phenomena do exist. A stellar example is Soal himself.

But the skeptics have not all been won over. In a paper entitled "Science and the supernatural," which we published on 26 August, George Price argued that some of the most widely accepted results could be duplicated—and hence could have been obtained—by fraudulent means and challenged the supporters of extrasensory perception to a "fraudproof" test.

Immediately we began to hear from readers. Angry letters told us how bad Price was for writing the article and we for publishing it. Congratulatory letters praised author and editor for their courage in frankly facing a difficult problem that sooner or later had to be explicitly handled. Letters in a milder tone elaborated one or another aspect of the controversy. Of all the letters submitted, we selected four for publication. They appear in this issue, together with Price's reply and a rejoinder by Rhine to Price's reply. These half-dozen papers mark the end of this particular episode, but no one expects them to end the debate on extrasensory perception.

The central issue is whether or not the possibility of fraud has been ruled out. Although each side offers strong arguments, neither is fully convincing. In the opening paper, Price argued that the results reported by Rhine and Soal are incompatible with accepted scientific principles and their vast body of supporting evidence. Price then proposed to explain those results by introducing the additional hypothesis of intentional or unconscious fraud. However, a contradiction between ESP reports and accepted science implies only that the truth of ESP claims is highly improbable, not that the claims are necessarily in error. What appears improbable on the basis of one body of evidence may prove to be quite probable when fresh evidence is discovered.

In the closing note of the exchange, Rhine argues that the fraudproof experiment proposed by Price would demonstrate nothing. Failure to reproduce ESP would show only that it is not a phenomenon that can be summoned at will. However, by summoning a little patience, science can deal with rare and ephemeral phenomena. One cannot reasonably expect to witness a rainbow or an earthquake whenever one chooses. If ESP is a fact, evidence should appear after trying a reasonable number of subjects and experiencing a reasonable number of failures, even under conditions guaranteed to exclude fraud.

However the ESP debate eventually comes out, certainly it is not yet finished. As it goes on, perhaps we should keep in mind the comments of Donald Laird, who wrote: "To avoid deadly seriousness and bitterness, it might be well to remind ourselves of the undergraduate who interpreted the initials ESP to mean 'error some place.'" Skepticism is still in order, as is, on both sides of the argument, the most rigorous effort to exclude both intentional and unintentional error.—D. W.

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On "Science and the Supernatural"

S. G. Soal

I have read with some amazement the article "Science and the supernatural" (1). In this paper George Price suggests fraudulent collusion between the chief experimenter (presumably myself) and a number of highly respectable people as an explanation of the significant results obtained in the card-guessing work carried out with Basil Shackleton and Gloria Stewart reported by F. Bateman and myself (2). Moreover, Price makes these suggestions without being able to produce the least fragment of factual evidence that any such fraudulent malpractice ever took place. It is, I think, safe to say that no English scientific journal would have published such a diatribe of unsupported conjecture. *Nature*, the leading English scientific weekly, has nothing but praise for our work, in a recent book review (3).

Price begins by saying that "In his early work as a psychic investigator, Soal published excellent papers reporting negative findings and showed himself to be a meticulous and ingenious experimenter, expert at uncovering trickery." But every competent critic has admitted that the Shackleton experiments, for instance, were on a higher level of technical efficiency than any of the earlier 1934-39 card-guessing experiments. In the earlier work, for example, the guesser and sender were in the same room separated only by a screen, whereas elaborate precautions were taken in the later work to eliminate all sensory cues. Apparently

Price considers the early experiments to be "excellent" merely because they produced only negative findings. In much the same way critics hostile to extrasensory perception pronounced Coover's (4) very defective experiments to be "a notable example of painstaking, thorough research and exact treatment of numerical data" (5). There is little doubt that if Coover had obtained positive results of high significance his experimental methods would have been described in far less flattering terms.

It is very significant and somewhat comforting to learn that Price admits that "most of Soal's work" cannot be accounted for by any combination of statistical artifact and sensory leakage. He is convinced, for instance, of the inadequacy of Rawcliffe's theory of "double whispering" in disposing of the Shackleton results (6) or of Spencer Brown's suggestion (7) that the extrachance scores are due to nonrandomness in the target series or to defects in probability theory (2).

He is therefore driven, as a last resort, to suggest that the experimenters have deliberately organized fraudulent techniques that have been successfully practiced in the case of Mrs. Stewart over a period of 4 years without detection by the numerous academic people who have taken part in the experiments. In taking this attitude Price would appear to be trading on the prejudice and hostility that a majority of American scientists bear toward the subject of telepathy. In England the attitude of scientific men and philosophers is far more tolerant and open-minded, and such an attack as that

of Price would be considered grossly unfair unless he could produce actual evidence that cheating had taken place.

Price has suggested several methods by which the experiments could have been faked. I propose to examine these suggestions in some detail.

In at least three of the procedures described the Agent or sender and the Percipient (as well as EA, the chief Experimenter) are in the trick. The Agent, sitting behind the screen arranges the five animal cards in an order that has been decided beforehand by EA. Or in another variation the Agent lays out the cards in any order and communicates this order to EA on the other side of the screen by means of some code concealed in a phrase such as "I am now ready." EA then communicates this order (or certain partial constituents of it) to the Percipient in the next room by means of a code contained in some commonplace phrase or by means of inflections of his voice, and so forth. The Percipient who is in collusion with EA has previously memorized certain numbers chosen by EA from certain key positions of his list of random numbers. As EA calls aloud the serial numbers of the 25 guesses, the Percipient decodes the numbers in the key positions into the corresponding initials of the animals' names.

Price goes to great length in devising variations on this theme, but they all depend on the Agent being in collusion with the chief Experimenter or with the Percipient. Now four of the Agents with whom Mrs. Stewart was highly successful were lecturers of high academic standing at Queen Mary College in the University of London. Two were senior lecturers and the other two were mathematicians who had done distinguished creative work. A fifth Agent who was brilliantly successful over a long period was a senior civil servant, in fact an assistant director of mathematical examinations in the Civil Service. Now is it plausible to suppose that I, as chief Experimenter, could persuade any of these men to enter into a stupid and pointless collusion to fake the experiments over a period of years? What had any of them to gain from such deplorable conduct? If I had gone to any of them and suggested (as Price recommends) that in a good cause a little deception would do

Dr. Soal is a research fellow in parapsychology at Birkbeck College (University of London) and a past president of the Society for Psychical Research, London.

no harm, I know quite plainly that the result would have been a first-class scandal in university circles. These men had no burning desire to prove extrasensory perception and no religious axes to grind. They had everything to lose by besmirching their academic reputations. Their only motive was scientific curiosity. It is idle, therefore, for Price to assume that these five Agents would consent to arrange the cards at the bidding of myself or deliberately to communicate the code either to me or to the Percipient, Mrs. Stewart. Certainly, one might find obscure people with no conscience who would, if they were paid for doing it, assist in faking an experiment, but not in the ranks of University of London lecturers.

If then, these Agents were not in the trick, how did EA get hold of the code in order to communicate it to Mrs. Stewart? Since in many such experiments another academic man was sitting by Mrs. Stewart handing her numbered record sheets to fill in one by one, it would be clearly too late for her to receive the code after her 50 guesses had been completed. Nor could she draw prepared lists of guesses from a drawer, since there was no accessible drawer at the table where she sat, and even if there had been one her every movement was under observation by the academic man sitting beside her. EA might, of course, ask the Agent innocently for the order of the code at the commencement of each run of 50 guesses, but all 30 Agents would swear emphatically that no such thing ever happened and that during a run EA never left his own side of the screen. Moreover, asking for the code would excite immediate suspicion. Price has made the suggestion that EA, looking through the hole in the screen, might see the reflection of the five cards in the Agent's spectacles. But with the lighting of the room as it was and the position of the hole and the size of the box, it can easily be verified that such a thing would be impossible. I have always been on guard against reflections in card experiments, and since the main object of my setup was to insure that EA who gave the signals to Shackleton or Mrs. Stewart should have no knowledge of what card the Agent was looking at, I naturally took special precautions to see that reflections in spectacles, window panes, and so forth, were impossible. I am ready to demonstrate to anyone that the spectacle theory is an erroneous one under our particular conditions.

If then the Agent is not in the trick, it would appear to be impossible for the code to have been communicated to Mrs. Stewart until she had recorded her guesses. I could cite large numbers of highly successful experiments in which

both the Agent and the person who sat with Mrs. Stewart were people of academic standing. Let me give only two examples.

At sitting No. 52 on 23 April 1948, Louise Morgan (2, p. 225), a well-known journalist on the staff of the *News Chronicle*, visited us for the first time and took part as Agent. Brendel of Queen Mary College sat by Mrs. Stewart for the whole time while she was making her guesses. The checking of scores was done by Brendel, watched by Morgan and R. A. M. Kearney, a mathematician. Mrs. Stewart made a score of 109 hits in 400 guesses. This gives an excess over chance expectation of more than 3.5 standard deviations. Now no one will suggest that I could be such a fool as to attempt a collusion with Morgan. If I had done so I should have seen my name in letters of infamy in next morning's *News Chronicle*.

And here is an experiment in pure telepathy (2, pp. 252-253) in which Rozelaar of Queen Mary College was the Agent. In this case no actual cards were used, but the Agent imagined a code to be printed on five blank pieces of paper and did not divulge it until Mrs. Stewart's guess sheet was safely in the hands of Bateman (assistant director of examinations to the Civil Service Commission) who sat by Mrs. Stewart. In 200 trials she obtained 60 hits—the equivalent of 3.5 standard deviations. Here there was no question of EA (myself) reading the code in Rozelaar's glasses. (Actually at that time he did not wear spectacles). And as I have said it would be absurd to suppose that a senior lecturer of the University of London would lower himself to assist in faking an experiment. Rozelaar had no connection whatever with any psychical organization. The guesses were decoded by Bateman and checked by Mrs. Hales (a highly respectable professional pianist), and Rozelaar himself checked me as I called aloud Mrs. Stewart's guesses.

I could multiply examples of experiments of this kind. Moreover, Mrs. Stewart was successful with 15 Agents out of 30 that were tried. Price's assumption of collusion between myself and fellow-lecturers at the University of London has no basis in reality and is a fantastic product of his own imagination. Many people would consider such a hypothesis to be more improbable than the existence of telepathy itself, for which there is a vast amount of spontaneous evidence of good quality quite apart from card-guessing. Indeed in formulating his themes of collusion, Price has not taken sufficiently into account the high quality of the personnel connected with these experiments. Nor has he any acquaintance with the mentalities of the Percipients themselves. No

one, for instance, who knew Shackleton would credit him with the ability to memorize accurately certain random numbers located in varying key positions in as many as 12 or 16 columns and, in addition, to transpose these numbers into code letters at the rate of one every 2 seconds. I should experience the greatest difficulty in performing such a task myself, even at the normal rate of calling, and at the rapid rate of a call every second I should find the thing impossible. With an observer watching every movement, I should be unable to pull from my pocket any lists with which to refresh my memory. And to have to carry out such a nerve-racking performance week after week would be intolerable.

Then again the reproduction of the many subtle position effects described in Chapter XIX of *Modern Experiments in Telepathy* would be very difficult to fake.

In certain of the Shackleton experiments the lists of random numbers were prepared by Wassermann, a mathematical physicist, and I had no opportunity to see them until the experiment was over. Most people in England who know Wassermann would have little doubt about the sort of reaction that would be induced in him by a request to assist in faking an experiment!

Price evidently thinks that extrasensory perception should be established once for all by an absolutely fraudproof, cast-iron experiment. The late F. C. S. Schiller, the Oxford philosopher, used to argue that such a hope was illusory. Even if such an experiment were feasible, we should find that, as the years passed and the experiment faded into history, fresh doubts would begin to be raised about the reliability of the experimenters or the possibilities of collusion.

Another experiment would then be necessary, and the arguments would begin all over again. On this question I am in agreement with Schiller, and I favor a quite different method of approach.

The main obstacle to the acceptance of parapsychological phenomena is the apparent rarity of the people who can produce them under even reasonable conditions of control. Now this rarity I believe to be apparent rather than real. We do not know the signs by which to distinguish these exceptional card-guessers and so we waste time and effort in testing the wrong kind of people. There is increasing reason to believe that we shall not discover them in university populations and that it is a waste of time to experiment with students. But experience of the last few months has indicated that it is among the less sophisticated types that we should pursue our search—especially among children living in rural communities or in backward countries.

I think there is little doubt that with

an increasing number of such high-scoring subjects much of the prejudice of ordinary scientific workers will disappear. When more and more competent Experimenters report on cases of high-scoring subjects, the hypothesis of collusion will become as extinct as the dodo. While it is, in the last resort, possible to suggest that two or three Experimenters have faked their results, this will not be possible when scores of competent investiga-

tors produce their reports on similar cases. I suggest to Price, therefore, that efforts should be directed toward the discovery of the personality characteristics of these people who make averages of 8 or 10 hits per 25 over considerable periods, the sort of communities in which they may be successfully found, and so on. In other words we should aim at repeatability by more and more investigators.

Comments on "Science and the Supernatural"

J. B. Rhine

Credit Side

Strange though it may seem, the publication of the George Price paper, "Science and the supernatural," is, on the whole, a good event for parapsychology. It is not merely that it is better to be attacked than it is to be ignored. According to the ways of American science, a revolutionary finding has to be cuffed and kicked through the entrance in order to gain admittance. When unorthodox issues are concerned, only critical articles, and the rougher the better, are likely to be accepted by the scientific periodicals. In fact, one can easily fancy (as some readers have) that Price deliberately undertook to sell parapsychology to American science by disguising a really informative article as a slanderous critique, with charges so utterly exaggerated that they would not be believed even by skeptics of ESP. At any rate, as a way to get a lot of instruction on parapsychology into *Science*, it worked as well as if it had been planted.

It is also of value to parapsychology to have Price portray so vividly the potential importance of psi abilities. He has even more clearly appreciated the great potential applications of ESP than have many of the workers in the field. It is

true that he has overlooked the limitation owing to the unconscious level on which this elusive function operates; but if (as is not unreasonable to expect) that limitation can be overcome through future investigation, his picture of the utility of psi will be entirely realistic.

Again, credit goes to Price for his coverage of the older criticisms of the psi research. Although they have been answered many times in the literature of parapsychology by others, Price has summed up the case rather well—so well, in fact, that but for the philosophical blockage from which he reveals he suffers, he sees nothing to prevent the acceptance of ESP. It is true that, rather than to question the mechanistic philosophy that he recognizes is at issue, he oddly professes to believe that all parapsychologists are liars and montebanks; but such a wild charge, even if Price really intended it to apply to the dozens of university and other scientists involved, is not likely to be taken seriously. On the other hand, his effective answers to the earlier criticisms of ESP work will and should carry weight with them. In a word, he has himself rounded out a fair case for ESP for all but the utter cynics who can accept his fantastic suspicion of a vicious conspiracy among academic research workers and a monstrous half-century-long hoax.

Finally, and best of all, comes the point that most concerns Price himself.

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He has focused more neatly than any other reviewer the deadly, menacing sting of the psi research findings. It is of great importance, indeed, for parapsychology to have the point of this issue brought out sharply and clearly in the pages of *Science* itself! I myself, in a voice scarcely audible in conventional science, have been shouting from the housetops the very same issue that Price has drawn. It is the head-on collision between the facts of parapsychology and the prevailing physicalistic theory of man (or call it mechanism as he does, or materialism, or physical monism, or what-not). The fact is that this philosophy, on the one hand, and these experimental facts, on the other hand, directly contradict each other in an inescapable, horn-locking manner. Walker (1) and Boring (2), among others, while they have sparingly admitted in recent publications that there are some experimental results in parapsychology that have to be dealt with, have failed to see the lethal blow that these research results give to the belief in physicalism that both authors espouse. They hold out, rather, for some future, more elastic, physicalistic concept that may eventually account for these puzzling findings of today.

Ignoring his language, I prefer Price's forthright demand for the balancing of the books right now. He, even more than any other critical reviewer, gives indication of having felt the force of the evidence for ESP. When he turns then—albeit a bit too emotionally—and says that, according to the current concept of nature, ESP is impossible and therefore the parapsychologists must all be fakers, he at least draws the issue where it can be squarely met. The answer of the parapsychologist is: "Yes, either the present mechanistic theory of man is wrong—that is, fundamentally incomplete—or, of course, the parapsychologists are all utterly mistaken." One of these opponents is wrong; take it, now, from the pages of *Science*! This recognition of the issue gives point to the findings of parapsychology in a way none can easily miss.

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Mechanistic Assumption versus Experimental Facts: the Setting

Need I ask now—above all, in *Science*—what a scientist should do when a metaphysical doctrine such as this mechanistic philosophy of man is contradicted by a set of experimental results? It is surely part of one's elementary training that one proceeds as Newton, Darwin, Pasteur, and others have exemplified in all the sciences of nature. Generally speaking, the scientist concentrates on the reexamination and confirmation of his facts until, if they bear up under these demands, the opposing belief itself gives way and a modified philosophy of nature develops—one that accommodates itself fully to the new discoveries. If, on the other hand, errors are found, they are specifically exposed and that ends the matter; but, as Price himself has explained, the better ESP work has not been successfully attacked on that score.

It may make the facts of the psi investigations more understandable to retrace from the beginning, at least in outline, the course over which the inquiries in parapsychology have progressed (3). (We might also watch for any magic or supernaturalism along the way!) It is a course typical of the introductory history of any natural science. The investigations first arose, as in most other sciences, because of spontaneous natural occurrences. In this case, certain puzzling and unclassified human experiences started the whole inquiry. These experiences suggested that there might be a way of communication by the mere transference of thought; this was eventually called telepathy. Such communication had not appeared to be very reliable, however, and hence its possibility was neglected as unimportant until a stage in Western culture had been reached at which circumstances gave it increased importance. This new significance had nothing to do with practical utilization and, accordingly, a high order of reliability was not important.

Rather, it was its bearing on the theory of man that brought telepathy to the attention of science. When, in the latter half of the 19th century, materialism severely challenged the traditional spiritual view of man, there were those who thought that the claims of telepathy ought to be looked into because they suggested that a transfer of thought could occur between persons without physical intermediation. Such an operation was taken as mind-to-mind contact transcending the scope of physical explanation. It seemed, therefore, to constitute a challenge to the claim of materialism as a complete theory of human life. Hence it was the very issue that upset Price that led to the rise of parapsychology in the first place.

Eventually—by the 1870's and 1880's—experiments in telepathy were conducted and reports of them got into print. These were criticized and in due time new ones with methods modified to meet the objections were carried out. These in turn were published and received criticism, and so the cycle of all exploratory science progressed. It was not, however, until the second and third decades of the present century that the study of telepathy and extrasensory perception in general began to gain a foothold in university laboratories. But with more concentrated studies in the 1930's and 1940's, finally the professions most concerned were more or less compelled to take notice of the researches in ESP. National organizations of psychologists, statisticians, biologists, and certain medical groups in Europe and America held symposia for the appraisal of the results and claims of parapsychology. The case for ESP not only stood the test, but even more, its status gradually improved. Today, even if the only criterion were Price's type of article, it would be safe to say that ESP is making its way. Parapsychology now occupies an officially recognized place in a half dozen or more leading universities of the world, ranging from fellowships to professorships and from lectures to laboratories.

What Evidence Has Mechanism?

If, then, it was to refute the mechanistic philosophy that parapsychology arose, it is not enough for the inquirer to consider only the evidence of psi. He needs also to ask: How strong a case has been made for the philosophy that opposes it? What evidence is there for a belief in the complete dominion of physical law over man and nature? As a universal law, this claim has never had any truly experimental confirmation whatsoever. How in the nature of things could it have? Actually, this whole mechanistic business means only that in those areas of nature in which most of the scientists of the world have been working—the various physical sciences—physical theory has been adequate. Naturally. Consequently, mechanism just grew like Topsy and became a habit of mind, a way of looking at the universe. It has even proved successful in dealing with the surface problems of the fields of biology. In the more physiological areas of psychology, too, it has had great success. But to establish that this physicalistic interpretation applies to the whole of nature and that there are no other kinds of principles in the universe would call for a complete understanding of nature. Of course, we have nothing like that, as everyone well knows. As I have said, mechanism is just a habit of mind.

Even one single well-established exception would disqualify a philosophy that is assumed to be an explanation of the entire universe. In particular, any thoroughly physicalistic theory is completely defenseless against such an exceptional case as parapsychology, by its very nature, represents. Recalling, too, that the biological and psychological sciences are still far—and exceedingly far—from explaining most of the basic functions of life and mind, reminds the student of how far present knowledge still is from the final authoritative and incontestably complete view of nature that these hard-shell mechanists take for granted. When anyone gives to such a belief the almost dogmatic finality that Price apparently does, it suggests that the doctrine has taken the place of a security-giving theology and is playing more than a scientific role in his life.

Naturalistic Approach of Parapsychology

Unlike the opposing philosophy of physicalism, the position of parapsychology rests wholly on experimental evidence. These psi phenomena are empirically observed biological effects; they are, more specifically, verified psychological occurrences and they have been strictly and objectively demonstrated to be a part of the natural functions of the individual. Moreover, they are lawful and, as research has revealed their conditions and properties, they make sense against the larger background of human and animal life. In fact, the ESP results warn the scientist that again the boundaries of knowledge have been drawn too close and that once more they will have to be extended. Over and over in the past, that has been the way in which the scientific map of the universe has developed.

It is because it *does* fall well outside the present boundary of conventional science that ESP is a challenge to the science of today. Its advantage is that it is an operation of the personality—the only one thus far known—that can, in a controlled experimental manner, be shown to operate with a certain independence of the physical order of nature. Thus it becomes a sort of key to a farther zone of reality that is identified with that least understood of the natural divisions, personality.

In showing effects independent of the time-space criteria of physical nature, the psi function requires the inference of an underlying energetic operation—one that is neither intercepted by the physical end-organs of the sensory system nor limited by the physical conditions that affect the more familiar energies. Yet, as the discoveries in psychokinesis have

shown, this inferred mental energy, if we may call it that, is convertible into a measurable kinetic operation. Moreover, a lawful and rationally consistent picture is emerging bit by bit from exploratory studies now going on. The results are proving to be organizable and their relationships are proving to be logically coherent. Nonphysical though psi appears to be as judged by the familiar criteria of space and time, it is, nonetheless, a natural function of the normal personality, a part of the living organism and as much a part of the process system of nature as anything already in the books. Its properties, as far as they are known, have been studied by the standards and methodology of natural science in general. No favors have been asked and no concessions are needed that are not claimed by general psychology or genetics or nuclear physics or any other branch of research.

The extraordinarily hidden character of the operation of psi, however, has made its practical utilization difficult. It has also made its scientific demonstration a tedious and long-drawn-out affair. The elusive character of this deeply unconscious function of the personality still gives serious trouble in the research laboratory, as the literature amply recognizes. In fact, unless and until psi ability can be made subject to conscious control, or a device for releasing it on the unconscious level can be developed, it is hard now to see how to bring it to the fruition of ready application that Price outlined. But in any case, no claims are made in advance. The same deeply buried character of the psi function suggests, along with other indications, that psi ability has had an early evolutionary origin. Moreover, a vast storehouse of animal behavior—for example, homing—has been found lying recorded but unexplained since the days of Darwin; this behavior suggests a rich field of possible psi manifestation and application.

The brief outline I have given of what has been done in the investigation of ESP and in the interpretation of the results has been presented in order to "naturalize" it for readers who may have misconceived the whole field as an unrealistic, occult business. It can be seen, however, that except for the fact that parapsychology has turned up a type of phenomena strange to the conventional sciences, the course of development of this branch of science has followed that of a typical naturalistic and objective inquiry. The new facts themselves actually fit well enough into the systematized knowledge already familiar. The clash—and there is an unmistakable one—is only with the wholly theoretical philosophy of materialism. There, however, the conflict is complete and inescapable. That, of course, is what makes the findings of

parapsychology controversial. It is also what makes the findings of parapsychology revolutionary.

Price's Objective

This is, I trust, my last reply to criticisms of ESP—the last of a 20-year series—for Price has evidently "thrown the book." As he well indicates, however, there was nothing much left to say against the evidence of ESP when he took over; nothing, that is, but a few last resort name-calling, and now he has used up that reserve. On this level of discourse, he may have the last word for whatever it is worth to history. Some readers of Price's article, however, who are not familiar with parapsychology, may incline to take his objective too seriously. For them I have a few remarks.

Actually, his article appears to me rather more like an *act* than a genuine earnest critique. Take, for example, the charge of fraud—one that would ordinarily be a matter of dead seriousness. It is fairly obvious, I think, from the record alone that Price did not really believe what he tossed off without pretense of proof. Had he honestly believed there was fraud, the same impulse that led him to write this article would almost certainly have impelled him to dig up some tangible evidence concerning at least one parapsychologist. Such investigations have actually been made more than once in the past by persons who sincerely wanted to know.

Again, the experimental proposal Price flung at the parapsychologists looks just as much like a pose as the character-knifing act. After first declaring ESP philosophically so completely impossible as to justify his wholesale suspicions of fraud, he then proceeded to demand that the parapsychologists nevertheless conduct a fantastic new experiment of his designing—one that would, he implies, convince him if it gave positive results. The latter sounds open-minded, does it not? All he needs is evidence and the impossible would be possible. Price is either confused or else, as I think, he is proposing this experiment with his tongue in his cheek. Or does it matter? Certainly not so far as it concerns the values I have credited to the publication of this paper.

There were such challenges as Price's in the 1930's. The now classic Pratt and Woodruff (4) experiment in ESP was carried out to meet one that was made jointly by seven American psychologists. Pratt and Woodruff exceeded the precautionary standards submitted and their experiment was successful too; but the net effect on the seven psychologists was that it merely silenced them; it did not convince them. S. G. Soal, who was especially singled out for suspicion by Price, was himself at one time one of the lead-

ing challengers of the Duke experiments. His own negative results from years of work proved, however, on reanalysis, to contain significant evidence of ESP, and the conversion of Soal from critic to colleague stopped a lot of criticism of ESP in the 1940's. Knowing all this as he must, Price can hardly be entirely serious in his talk of conspiracy in the Soal experiments and his demand for a so-called fraud-proof experiment.

Perhaps it is enough to suggest rather that ESP has now approached the status of "big game" and one may not need to search for any other purpose than that of the hunter's impulse to bring it down. Whatever the motive, the value of the publication of Price's article in *Science* stands out well above any other consideration whatever, and it would be a mistake to overlook this outstanding service to parapsychology in the consideration of minor defects. The designing of the experiments for that field can perhaps be left to those with more experience. But it took Price, whether trophy-hunter or sincere scientist, to get nine pages on parapsychology into a lead article in *Science*, with the crucial challenge of that field sticking out like a sore thumb.

American Way of Science?

This final point is not made on behalf of parapsychology but is beamed at science in general. It is something that I have culled from my prolonged participation in what has probably been one of science's fiercest controversies. Naturally, I have had to wait to mention it for the time when parapsychology was safely through its ordeal. Has that time come? Price could hardly have kicked a dead horse (or even a very sick one) through nine full pages of the world's leading scientific periodical.

American science, I am convinced, badly needs a forum—a journal that is open to new work, however radical its implications, without the usual practice of waiting for a savage attack to make it admissible. It is, of course, what might be called the forum attitude that is lacking. Yet it will be freely recognized by all that fair and unhampered presentation of revolutionary ideas and discoveries is especially vital to the continued advancement of inquiry. The national interest itself obviously requires the active cultivation of unrestricted investigation. It seems likely that the well-known lag of American science (omitting technology) behind European contributions in the more fundamental researches of the last 50 years (for example, in psychology and physics) is due entirely to this one distinct difference, this greater inhospitality to novel and unconventional claims that prevails in the United States.

Through the anxious years coming up, man's fitness to survive what already hangs over his head may easily depend on how well and how fast his scientists can think. But who knows what this thinking is worth until it is known—until it is made readily available in the forum, the symposium, and the periodical? It is time, and it is urgent, to borrow from the engineers their successful practice of reaching out for, instead of fending off, novel claims and unorthodox discoveries,

of clarifying their status promptly and in general encouraging the creative turn of mind—and to extend this practice to areas beyond that of gadgetry and invention, areas that have to do with the understanding of man and the guiding values of life.

In this last section I have been attempting to say that Price's article is perhaps more revealing with regard to the need in American science for a more tolerant attitude than it is of the status

of the struggling young science of parapsychology on which it has made a curious, bludgeoning attack. Parapsychology can now take care of itself, I think, but what about American science?

References and Notes

1. R. Walker, *Sci. Monthly* 79, 1 (1954).
2. E. G. Boring, *Am. Scientist* 43, 109 (1955).
3. I will furnish, on request, a reading list to those who may wish to go over the course more fully.
4. J. G. Pratt and J. L. Woodruff, *J. Parapsychol.* 3, 121 (1939).

Compatibility of Science and ESP

Paul E. Meehl and Michael Scriven

As two of the people whose comments on an early draft of George Price's article on "Science and the supernatural" he acknowledged in a footnote, we should like to clarify our position by presenting the following remarks.

Price's argument stands or falls on two hypotheses, only the first of which he appears to defend. They are (i) that extrasensory perception (ESP) is incompatible with modern science and (ii) that modern science is complete and correct.

If ESP is *not* incompatible with modern science, then the Humean skeptic has no opportunity to insist on believing modern science rather than the reports about ESP. If modern science is *not* believed to be complete or correct, then the skeptic is hardly justified in issuing a priori allegations of fraud about experimenters even when they claim that they have discovered a new phenomenon that requires reconsideration of the accepted theories.

In our view, both of Price's hypotheses are untenable. Whatever one may think about the comprehensiveness and finality of modern physics, it would surely be rash to insist that we can reject out of hand any claims of revolutionary discoveries in the field of psychology. Price is in exactly the position of a man who might have insisted that Michelson and Morley were liars because the evidence for the physical theory of that time was

stronger than that for the veracity of these experimenters. The list of those who have insisted on the impossibility of fundamental changes in the current physical theory of their time is a rather sorry one. Moreover, unhappy though Price's position would be if this were his only commitment, he cannot even claim that specifiable laws of physics are violated; it is only certain philosophical characteristics of such laws that are said to be absent from those governing the new phenomena.

It is true that Price attempted to give a specific account of the incompatibilities between ESP and modern science, rather than relying on Broad's philosophical analysis, but here the somewhat superficial nature of Price's considerations becomes clear. Of his eight charges, seven are unjustified.

1) He claims that ESP is "unattenuated by distance" and hence is incompatible with modern science. But, as is pointed out in several of the books he refers to, since we have no knowledge of the minimum effective signal strength for extrasensory perception, the original signal may well be enormously attenuated by distance and still function at long range.

2) He says that ESP is "apparently unaffected by shielding." But shielding may well have an effect: the evidence shows only that the kind of shielding appropriate to electromagnetic radiation is ineffectual; since detectors indicate that no such radiation reaches the percipient

from the agent, this is scarcely surprising.

3) He says "Dye patterns . . . are read in the dark; how does one detect a trace of dye without shining a light on it?" The two most obvious answers would be by chemical analysis and physical study of the impression (which is usually different for different colors).

4) "Patterns on cards in the center of a pack are read without interference from other cards." The word *read* is hardly justified in view of the statistical nature of the results; however, this phenomenon is always used by parapsychologists as evidence against a simple radiation theory, which it is. But no simple radiation theory can explain the Pauli principle and one can no more refute it by saying "How could one electron possibly know what the others are doing?" than one can refute the ESP experiments by saying "How could one possibly read a card from the middle of the pack without interference from those next to it?" These questions are couched in prejudicial terms.

5) "We have found in the body no structure to associate with the alleged functions." Even if true, this hardly differentiates it from a good many other *known* functions; and among eminent neurophysiologists, J. C. Eccles is one who has denied Price's premise [originally in *Nature* 168 (1951)].

6) "There is no learning but, instead, a tendency toward complete loss of ability" a characteristic which Price believes has "no parallel among established mental functions." Now it would be reasonable to expect, in a series of experiments intended to show that learning does not occur, some *trial-by-trial* differential reinforcement procedure. Mere continuation, with encouragement or condemnation after *runs of many trials* can hardly provide a conclusive proof of the absence of learning in a complex situation. We ourselves know of *no* experiments in which this condition has been met and which show *absence* of learning; certainly one could not claim that this absence was established, *even if it had been established*, it would be very dan-

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gerous to assert that there is "no parallel among established mental functions." In the psychophysiological field particularly, there are several candidates. Finally, *even if it had been established and there were no parallel among mental functions*, there would be no essential difficulty in comparing it with one of the many familiar performances that exhibit no learning in adults—for example, reflex behavior.

7) "Different investigators obtain highly different results." This is the most distressingly irresponsible comment of all. ESP is a capacity like any other human capacity such as memory, in that it varies in strength and characteristics from individual to individual and in the one individual from one set of circumstances to another. The sense in which Rhine and Soal (Price's example of "different investigators") have obtained "highly different results" is when they have been dealing with different subjects or markedly different circumstances—for example, different agents; and exactly the same would be true of an investigation of, for example, stenographers' speed

in taking dictation or extreme color blindness.

There remains only statistical precognition, which is certainly not susceptible to the types of explanation currently appropriate in physics: but then it is not a phenomenon in physics. Even if it were, it is difficult to see why Price thinks that we properly accommodated our thought to the distressing and counter-intuitive idea that the earth is rotating whereas we should not accept precognition. His test for distinguishing new phenomena from magic is hopeless from the start ("The test is to attempt to imagine a detailed mechanistic explanation") because (i) it is of the essence of the scientific method that one should have means for establishing the facts *whether or not* one has already conceived an explanation and (ii) it would have thrown out the Heisenberg uncertainty principle and action across a vacuum—that is, nuclear physics and the whole of electricity and magnetism—along with ESP.

Finally, Price's "ideal experiments" are only Rube Goldberg versions of the standard tests plus a skeptical jury. The

mechanical contrivances would be welcome if only parapsychologists could afford them, and the jury is obviously superfluous because, according to Price's own test, we should rather believe that they lie than that the experiments succeed. However, in our experience, skeptics who are prepared to devote some time and hard work to the necessary preliminary study and experimenting are welcome in the laboratories at Duke and London. Without the training, one might as well have (as Price would say) 12 clergymen as judges at a cardsharp's convention.

The allegations of fraud are as helpful or as pointless here as they were when they were made of Freud and Galileo by the academics and others who honestly believed that they must be mistaken. They are irresponsible because Price has not made any attempt to verify them (as he admits), despite the unpleasantness they will cause, and because it has been obvious since the origin of science that any experimental results, witnessed by no matter how many people, *may* be fraudulent.

Probability, Logic, and ESP

P. W. Bridgman

The recent article by G. R. Price in *Science* [122, 359 (26 Aug. 1955)] entitled "Science and the supernatural" directs renewed attention to a situation that doubtless has given many people, including myself, a feeling of discomfort, to say the least. My own attitude was expressible in a paraphrase of Price's quotation from Hume to the effect that he would be unwilling to accept such phenomena as those claimed for extrasensory perception (ESP) unless he could be convinced that their genuineness would be less miraculous than the occurrence of fraud somewhere.

My own attitude did not seize on the possibility of fraud, although it seems to me that Hume's position is irrefutable; it seized, rather, on the way in which contemporary arguments for ESP depend on

considerations of probability. I felt somewhat vaguely that I would rather think that my understanding of probability is faulty than believe in the genuineness of ESP. My scruples against the use of probability arguments had nothing to do with the details of the calculation of the enormous numbers that represent the odds against the scores obtained in ESP tests. I was willing to take the word of the many technically competent persons involved that the grinding of the machinery by which these numbers were obtained had been according to Hoyle. My scruples went much deeper and were concerned with the logic of the application of probability concepts to concrete events.

It has long been apparent that there is something "funny" about the probabil-

ity situation. Probability rigorously applies to no concrete happening. If we calculate that the chance of throwing a 6 with a die is one-sixth, and throw the die and obtain a 6, there is no method whatever by which it may be shown that the chance "actually" was one-sixth. Yet the phenomena to which the probability calculations justifying ESP are applied are concrete actual happenings, many of them a matter of record in black and white.

My old feeling that the logical situation should be further explored was fortified by a recent occurrence that is the immediate occasion for this note. I was reading in *Science* [122, 471 (9 Sept. 1955)] a review of the recently published collection of 1 million random numbers, when it burst on me in a flash of illumination that random numbers cannot be published. For a set of random numbers is a set in which it is impossible to predict any subsequent number from the preceding numbers, or in which there is no connection between the different numbers. But the subsequent numbers may be predicted, if the set is published, merely by reading the published list, and all the numbers of the set are connected by being written together on paper. A list of numbers *obtained* by a random process might perhaps be published if we could answer the question, What is it that

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makes any specific process random? The list itself cannot give the answer, because, given only the list, the process by which the list was obtained cannot be reproduced; there are an infinite number of ways of generating any finite list.

Randomness is an ephemeral thing, having meaning only during the activity of generating the numbers, and passing into limbo with the consummation of the process. And its ephemeral meaning was a meaning only in a certain universe of operations—we could see no way of predicting the next number in terms of operations drawn from our repertoire. The repertoire was to a certain extent arbitrary, dictated to a large extent by our purposes of the moment. These purposes might, for example, dictate that we focus our attention on those aspects of the situation that can be expressed in mathematical language of an acceptable degree of simplicity. I suspect that the prospective users of the list of 1 million random numbers have in mind only such mathematical purposes and limitations, and that any remarks made here will not affect in the slightest degree the value of the list for them or diminish its sales.

The paradox inherent in the application of a probability calculation to any concrete situation is well brought out by a comment of Bertrand Russell, who remarked that we encounter a miracle every time we read the license number of a passing automobile. For if we had calculated the chance that we would see that particular number, the chances would have been millions to one against it. In what respect is the situation here different from that presented by a better-than-chance score in an ESP experiment? a score for which we may suppose that our preliminary calculation of the chances gave the same expectation value as the automobile number. The occurrence of the automobile number does not jar us, and we continue to put it down to chance despite the odds against it, whereas the ESP result does jar us, and we say that it could not have been chance.

There are several features here that demand comment. In the first place, we have to justify ourselves in not regarding the automobile number as a miracle. This justification we offer rather easily, although it might be difficult to give a logically rigorous defense of our justification. We might, for example, offer in justification a consideration of the distribution of the chance of occurrence of all possible numbers. Within limits, the chance for the occurrence of any one number is the same as that for any other—there is no heaping up of probabilities in favor of any one number or range of numbers. We could not have expected the particular number to turn up, but on the other hand, we are not surprised

when it does. We reflect that *some* number *had* to occur, and we let it go at that and think no more of it unless we are prodded. If we are prodded to tell exactly what we mean when we say "this past event was chance," we admit that there is no property inherent in the event by which we can verify that it actually was chance, and we seek for the meaning elsewhere.

We may seek the meaning of "was chance" in what we do about it. Now the paradoxical thing is that when we say "was chance" we do nothing about it—we have come to the end. The reason that we have come to the end is that consistency with our position forbids that we attempt to go further. If we went ahead and sought for an explanation or any sort of rational involvement, we would be stultifying our conclusion that the result was chance. For instance, if after seeing the automobile number and noting the state of issue, we begin to reflect on the relative number of registrations in the different states, we have abandoned our position that the event was a chance event. As long as we remain consistent and do nothing, we are safe, despite the fact that we have effectively changed our definition of chance when we pass from anticipation of an event to viewing its occurrence in the past. In fact, the operational meaning of "this *was* chance" involves our resolution to handle the situation just by doing nothing. And it is our resolution to do nothing that protects us from the logical punishment to which we would normally be subject for changing our definition.

These considerations, I think, make it particularly clear that the locus of chance is in ourselves, with strong involvements of "expectation" and "surprise," and that there is little that is "objective" about it.

Consider now the situation presented by the ESP scores. Unlike the automobile situation, there is here an enormous heaping up of probability in the neighborhood of a particular score (5 out of 25 for the conventional testing cards). We could not expect a score for which the adverse odds were millions to one, and we *are* surprised when it turns up. We cannot now say with the same cogency as before "there *had* to be *some* score," but instead we draw the conclusion that the result could not have been chance.

We have to ask what we mean when we say "this event was *not* chance." Since we have already made an attempt to tell what we mean when we say "this event *was* chance," we might be tempted to think that our new question is trivial and that its answer is implied in the answer we have already given. I think, however, and this is perhaps the crux of this

note, that this is by no means the case. "Was chance" and "was not chance" are not simply related to each other as two terms in traditional Aristotelian logic, subject to the rule of the excluded middle. Because what we do to give meaning to "was not chance" is not simply or obviously determined by what we do to give meaning to "was chance." Whether there is any necessary connection in logic between the meaning of these two expressions is by no means apparent. That they are connected in use is another matter. If the advocates of ESP were content to say "All I mean when I say 'the event was not chance' is that the event was not expected and surprised me," I think we could have no quarrel. But it would be almost humanly impossible to stop with such a simple statement, and the advocates of ESP have shown their humanity by not stopping, but have gone ahead and envisaged all sorts of consequences, consequences that would usually be implied in an Aristotelian, excluded middle, system. Thus we can imagine them saying "Chance events are subject to no formulatable regularity—the events of ESP are nonchance; therefore they are subject to some regularity," with the usual additional implication that we are in the presence of a new unknown faculty of the mind. It seems to me that the only justification for drawing such a conclusion in a non-Aristotelian system is to be found in the actual exhibition of some sort of pertinent regularity, and this, as far as I know, has not been done.

There is a deep-seated difference between the way in which positive and negative probability arguments are fruitfully applied in practice to concrete situations, to which we have seen that the concept of probability does not rigorously apply at all. If the situation is a positive one, which we can characterize by saying "here we have the play of chance," then we can draw fruitful conclusions from the mere statement, without going further. This is shown by countless examples, as in the tables of a life insurance company, or the kinetic theory of gases, or the theory of the atomic nucleus with its calculation of the best construction for a hydrogen bomb by the Monte Carlo method. But if the situation is a negative one, characterized by saying "here we do *not* have the play of chance," we have something radically different. Here we *are* compelled to go further, and fruitful application is not achieved until we succeed in exhibiting the regularity that we suspect. The detective who says "It was not chance that five murders were committed by the same technique" has said nothing until he exhibits the man who committed the five murders. Wanting the ratification of exhibition, the statement of nonchance is merely an in-

vation, or an incentive, if you feel that way, to further investigation. ESP, with its statement of nonchance, but with its utter failure to exhibit any regularities or to perform a single repeatable experiment, is the only instance of which I am aware in which a serious claim has been

made that nonchance should be capitalized simply because it is nonchance.

The situation covered by the word *probability* is a desperately complex situation, mostly of our own making and in our own minds, with a fragile and fleeting dependence on time, and never co-

herently connected with concrete "objective" events. I personally can now see so much here that needs to be thrashed out and clarified that I am unwilling to accept the genuineness of any phenomenon that leans as heavily as does ESP on probability arguments.

Where Is the Definitive Experiment?

George R. Price

Since I have already stated at some length my views on psychic phenomena (1), I am reluctant to engage in continued arguments that can in no way settle the basic issue. As I wrote in the concluding paragraph of my paper, "the only answer that will impress me is an adequate experiment." Nevertheless, some brief comments on the statements by Soal, Rhine, Meehl and Scriven, and Bridgman are in order.

The Basic Issue

The most important portion of "Science and the supernatural" was the section suggesting new experiments. My two colleagues at the University of Minnesota, Meehl and Scriven, are incorrect in stating that my argument "stands or falls on two hypotheses . . . (i) that extrasensory perception (ESP) is incompatible with modern science and (ii) that modern science is complete and correct." My argument stands or falls on the two hypotheses that (i) previous demonstrations of psi phenomena have not been convincing to most scientists and (ii) that it is possible to perform convincing experiments meeting all objections that parapsychologists have made to previous suggestions for public demonstrations.

The most significant points that the reader should notice about the present correspondence are (i) that neither Rhine nor Soal has in any way criticized my proposed tests as unfair or technically faulty, and yet (ii) both of them reject these suggestions. Why?

Soal rejects the suggestions on the grounds that the results would be only temporarily convincing. However, if skeptics were even temporarily convinced, then numerous additional experimenters would begin investigating parapsychology and evidence could continue to accumulate.

Rhine rejects the suggestions on the grounds that a similar challenge issued by seven psychologists (2) was successfully met in the past, yet the results convinced none of the seven. But this is not correct. Angier *et al.* wrote as follows: "It must be emphasized that in no program is it possible, in advance . . . to cover all precautions. . . . It is necessary, therefore, that there be the most competent possible supervision, as indicated in Section IX below." Section IX read:

"The experiment should, throughout, be under the direction and control of two or more psychologists who are regarded by members of the profession generally as competent in the experimental field. One of these superintendents must be on duty during every work period, and have actual oversight of the conduct of the tests.

"In view of the present situation, and the need of a definitive experiment, it is highly desirable that the experiment be set up under the superintendence of three psychologists, each from a different university."

The Pratt and Woodruff experiment (3) did not meet the conditions of Section IX.

Meehl and Scriven criticize the proposed tests on the grounds that "the jury

is obviously superfluous because, according to Price's own test, we should rather believe that they lie than that the experiments succeed." I cannot follow this argument at all. If people would believe the entire jury of twelve to be dishonest in preference to believing in psi phenomena, then logically Meehl and Scriven should recommend a much larger jury, instead of calling the jury superfluous.

Meehl and Scriven also state, "The mechanical contrivances would be welcome if only the parapsychologists could afford them. . . ." I cannot agree with this. The fact is that mechanical contrivances do not seem to be welcome to most parapsychologists. For example, in 1948, while Soal was still successfully experimenting with Mrs. Stewart, B. F. Skinner suggested that he use simple recording devices and other mechanical aids (4). Far from following these excellent suggestions, Soal contented himself with writing—as he describes it—"a calm, but perfectly devastating reply" (5). Secondly, I am quite sure that money can be raised for the sort of demonstrations that I suggested. If parapsychologists have special difficulty in raising money for their ordinary research, it is probably because of the peculiar rules of their game. It would similarly be difficult to raise funds for development of a uranium mine that never shipped any ore and that could be seen only by a special group of initiates.

Are there any crucial defects in my proposed tests? I can see possibilities for minor improvements—for example, using an inaccurate rather than an accurate timing circuit in the random number generator and letting the examining committee consist of seven parapsychologists and eight skeptics since a seven to eight ratio would appear fairer than the one to two ratio I previously proposed. But nobody has yet pointed out to me any important defect. To be sure, Rhine calls my proposals "fantastic" and Meehl and Scriven use the expression "Rube Goldberg." But what do such terms mean? If any of these men or anyone else has specific criticisms or sugges-

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tions for improvements, it would be kind of them to make the suggestions known.

In addition, I hope that some properly qualified person will volunteer to take charge of planning and arranging a definitive test, in the event that the parapsychologists change their minds and offer to participate in one. I would think that, primarily, such a person should be a scientist of high reputation; and it would be desirable (though it is not essential) that he be one who has in the past taken a public stand against parapsychology.

Miscellaneous Points

For Bridgman's views I have the utmost respect, especially because his writings have played a major role in the shaping of my own scientific philosophy. Nevertheless, I do not feel that his present probability arguments provide an escape from the dilemma of believing in extrasensory perception or in fraud. In a great deal of this work, Soal had his subjects alternate between telepathy and clairvoyance, and he found "extra-chance" results for the former only. In one run the Agent would know the identities of the 5 code cards (telepathy); in the next run he would not know their identities (clairvoyance). The clairvoyance runs consistently gave results in accordance with what standard probability theory predicts, and the telepathy runs gave quite different results. I do not see how this can possibly be explained on the grounds that there may be some basic flaw in our concept of probability.

It is interesting that most of Rhine's communication is devoted to stressing the incompatibilities between science and psi phenomena, while the Meehl and Scriven letter is largely devoted to arguing that there is no incompatibility. Of course I am on Rhine's side, in this matter and must resist the temptation to reply to Meehl and Scriven. It does not seem proper for me to use up more space in *Science* arguing a matter in which I am strongly in agreement with Rhine and Soal.

Soal has made a number of errors in describing my suggested procedures for cheating. For example, it is not correct that my procedures "all depend on the Agent being in collusion with the chief Experimenter or with the Percipient." The Agent was not necessarily "in the trick" in my Procedures 2, 5, and 6. For

imitating the Stewart series, with its 15 successful Agents, I would employ mainly Procedures 5 and 2. To imitate the experiments in the Shackleton series in which Wassermann prepared the random number lists, I would employ Procedure 4 where possible, and a variation of Procedure 5 if the Agent was being watched too closely to permit use of Procedure 4. Therefore, most of Soal's discussion of the honesty of this or that person is irrelevant. If Soal did cheat, it probably was not by procedures requiring intentional cooperation from Rozeelaar, Wassermann, or the four Agents from Queen Mary College.

Soal submitted a virtually identical statement to the *Newsletter* of the Parapsychology Foundation, and this statement, together with a more detailed reply from me, has already been published (6).

Rhine is in error in thinking that I "believe that all parapsychologists are liars and montebanks" or that I charged that "a hundred or more research scientists . . . [had indulged] in a gigantic hoax involving the hiring of confederates and such" (7). Outside of Soal's work, I do not believe that we are confronted with many experiments so excellent that we are forced to choose between ESP and fraud. But there are a few such cases.

Rhine, Soal, and Meehl and Scriven all complain that it was improper of me to discuss the possibility of fraud. Naturally I did this with considerable reluctance, but it was absolutely essential that this question be treated frankly in order to settle things one way or the other.

Rhine complains that I did not dig up "some tangible evidence [of fraud] concerning at least one parapsychologist." Now, of course, when it comes to phenomena so gross as to be apparent without statistical tests, there is available all sorts of evidence of fraud. For example, according to the March-April, 1955 *Newsletter* of the Parapsychology Foundation, the Society for Psychical Research, London, will shortly publish a 70,000-word report showing that the late Harry Price, one-time honorary secretary of the University of London Council for Psychical Investigation and author of *The Most Haunted House in England*, himself faked some of the evidence for the haunting of Borley Rectory. But in connection with phenomena so subtle as to be detectable only by statistical tests, my feeling was that it would be quite difficult

to prove in 1955 that A had whispered something to B in 1945.

Soal complains that I wrote "a diatribe of unsupported conjecture." But I did not. My conjectures that parapsychologists might be capable of fraud were supported by the eminent authority Soal himself (8):

"There is unfortunately among American investigators an atmosphere of showmanship which has created in the minds of British scholars a deep distrust. British scientists for instance are not favourably impressed by Rhine's discovery of a telepathic horse (or was it a precognitive clairvoyant pony?), by the sudden vanishing of Dr Reiss' phantom percipient into the blue of the Middle West, by the perfect scores of 25 cards correct in 25 successive guesses alleged to have been made by Pearce and the child Lilian, by the card-guessing feats of Pearce while sitting in a motor car and similar marvels.

"Such things simply do not happen in England, or if occasionally they appear to happen they are quickly exposed as frauds or conjuring tricks. In America they are not exposed; they are proclaimed genuine with a blare of trumpets."

Conclusion

Rhine has stated that publication of my paper is "on the whole, a good event for parapsychology." It would be wiser for him to see it not as a good event but as a good opportunity. This challenge has presented him with the opportunity to achieve at one stroke the scientific recognition for which he has been struggling for almost 30 years. But if he and Soal continue to evade the challenge, then publication of the paper will prove to have been a very bad event indeed for parapsychology.

References and Notes

1. G. R. Price, *Science* 122, 359 (1955).
2. R. P. Angier et al. *J. Parapsychol.* 3, 29 (1939).
3. J. G. Pratt and J. L. Woodruff, *ibid.* 3, 121 (1939).
4. B. F. Skinner, *Am. Scientist* 36, 456, 482 (1948).
5. S. G. Soal, *Proc. Soc. Psychical Research* 50, 67 (1953).
6. The September-October 1955 issue (vol. 2, No. 5) of the *Newsletter of the Parapsychology Foundation* (500 Fifth Ave., New York 36, N.Y.) contains statements by J. B. Rhine, S. G. Soal, G. R. Price, and D. Wolfe.
7. Statement by J. B. Rhine, quoted by R. K. Plumb, *New York Times*, 27 August 1955, p. 1.
8. S. G. Soal, *The Experimental Situation in Psychical Research* (Society for Psychical Research, London, 1947), p. 26.

I remember, one day, saying how uphill the work was, and he [Thomson] answered, "Yes, that is why there is so much credit in doing anything."—RAYLEIGH, The Life of J. J. Thomson.

The Experiment Should Fit the Hypothesis

J. B. Rhine

The trouble with Price's experiment is that it is based on an unwarranted assumption about ESP. He says in effect that if ESP really operated in the researches reported, it ought to register its effect dependably in his proposed test. He assumes that such a result would *have* to follow. Therefore failure would be fatal to the hypothesis of ESP, while success would (to him at least) acceptably prove the case.

Unfortunately, ESP is not like that. No parapsychologist has ever claimed that the capacity could be made to function on demand as Price assumes. ESP is still an elusive, uncertain, capacity, one that may give high scores one day and chance scores the next; it may persist in consistently missing its target or even hitting the neighboring one. The elusiveness is attributable to the fact that the ability, although voluntary, operates very largely on an unconscious level (1).

The same mistake was made by Price in his earlier discussion of the practical application of ESP; he overlooked the fact that ESP is not a push-button effect to be turned off and on at will, as a chemistry test might be. In fact, one could easily believe science fiction has been one source of Price's conception of ESP. He has fancied a kind of repeatability and applicability that as yet simply does not exist. It is premature to expect them in such a difficult field.

As far as the mere physical conditions of the proposed test are concerned, however, I see nothing wrong in principle. The test would not involve anything essentially new. Physical barriers such as the proposed metal containers are not obstructive to the capacity. Certain psychological conditions are, of course, essential—conditions such as adequate motivation on the part of the subject who is participating, confidence in his ability to work under the conditions, and freedom from distraction. But such conditions could probably be provided, and, for some subjects at least, proper adaptation to the test conditions could probably be

managed in the course of time. If this adaptation were the only difficulty and there were adequate reasons for the test, the procedure could be accepted and used, although, of course, with the same unpredictability of results that attends all ESP tests. As it is, with a false premise concerning the nature of the ESP process, Price made his test unacceptable by giving it an implication of a finality that it could not possibly have.

Price is, in effect, dictating terms to nature rather than to the parapsychologists. Until someone claims to have exerted enough control over ESP to bring it reliably into operation on demand, such a test case or showdown as he suggests is, as I have said, fantastic. The point is that negative results would prove nothing at all. Until (if ever) ESP becomes controllable enough to warrant such a crucial test case, it would not be worth while going to all the build-up and expense that the proposed experiment would involve.

The principal aim of Price's proposed experiment is to exclude fraud. But he needs only to remember that science has, in the very nature of its procedure, protected itself against such weaknesses as that. When any revolutionary claim such as ESP is reported, the cautious scientist will naturally suspend judgment until an independent confirmation has been produced. On a very challenging issue a second or even a third supporting research is needed. The extreme skeptic may, of course, keep on suspending decision as long as he wishes. But when, as in the case of ESP, researches continue to come in, adding confirmation upon confirmation, decade after decade, from investigators in all sorts of professional and academic stations, only those who are extremely biased would cling to a theory of wholesale deception.

The spontaneous, uncontrollable nature of ESP naturally bothers us all, parapsychologists as well as skeptics. But many other erratic, fugitive effects can be found in nature, more especially in

the mental sciences, but even in biology and physics. And they are no less "natural" than the more reliable ones, for all man's inability to reproduce them at will. Control is usually just a question of further understanding of the phenomena concerned. In the case of ESP, lack of control is likely to be nothing that more and better researches will not correct.

In the meantime, scientists who openmindedly wish to satisfy themselves about ESP have two main lines of action open. The preferable way would be to ascertain the essential precautions and psychological conditions that are already known and to conduct an exploratory ESP experiment, as many others have already done. Parapsychology owes much of its evidence and most of its eminent supporters to just such exploratory investigations.

The other way is, of course, the one more generally followed in science. It begins with the critical appraisal of the research literature of the field. This literature is vastly more extensive and important than the few names given by Price indicate. Indeed, all the work reported by Soal and myself (the two "exhibits" that Price used) could be set entirely aside without seriously weakening the case for ESP or even involving the very best controlled experiments (2). During the last twenty years there have been scores of researches reported (mostly in the *Journal of Parapsychology*) that have adequately met a standard of requirements of safeguarding (even against fraud) well above that of science in general. Let anyone who is able and willing critically review the evidence for ESP to show cause, if he can, why and wherein these most qualified investigations should not be taken seriously! The *Journal of Parapsychology* will be open, as always, to the publication of such reviews.

Price has, I repeat, done parapsychology much good, as, for example, in neatly showing the fallacy of Bridgman's type of criticism. His crusading against evil-doing in ESP only serves to make his blows against its critics more effective. It even helps to unbar the portals of respected periodicals. If this is the way a research field has to be opened up to broader scientific attention here in the United States, we in parapsychology must be willing to pay the price and be grateful for the net gain.

Meanwhile, then, the scientist can determine by the usual methods how far it is safe to credit the ESP reports.

References

1. J. B. Rhine, *J. Parapsychol.* 10, 162 (1946).
2. J. G. Pratt and J. L. Woodruff, *J. Parapsychol.* 3, 121 (1939).

News of Science

NBS Jerkmeter

■ The National Bureau of Standards has devised a barium titanate jerkmeter for studying the action of the human heart. The instrument is essentially a piezoelectric accelerometer with an output that is electrically differentiated. Unlike most instruments used in ballistocardiography, the jerkmeter does not require a fixed reference point. Thus, it is inherently isolated from extraneous motions such as building or operating-table vibrations. The device was designed and constructed by T. A. Perls and C. W. Kissinger under a program of basic instrumentation sponsored at the NBS by the Department of Defense and the Atomic Energy Commission.

In general, a jerkmeter is a transducer that gives an electrical output proportional to jerk, the time derivative of acceleration. Jerk has been measured in connection with elevator and vehicle riding comfort as well as in various physiological studies. The present development was undertaken to obtain measurements of the third time-derivative of the displacement of a reclining patient. The motion of interest is caused by the inertial forces generated by the flow of blood and is therefore intimately related to the function (and malfunction) of the heart. Developed as a research tool, the jerkmeter is being used by the Civil Aeronautics Administration to study the correlation of jerk measurements with proper diagnosis of heart conditions.

Creation of Rare Minerals

Rare minerals seldom found near the earth's surface have been created at the University of California, Los Angeles, by subjecting common minerals to extreme pressures and temperatures. Using a laboratory device called the "simple squeezer," George Kennedy and David Griggs of U.C.L.A.'s Institute of Geophysics have duplicated conditions that form minerals at extreme depths in the earth's crust.

From common quartz they have created coesite, a dense mineral that can exist in nature only at a depth of 40

miles or more in the earth. They have made jade from feldspar, and aragonite from limestone.

The research team also has been able to make various dense aluminous minerals from ordinary clay. From data on temperatures and pressures required to make these minerals, the depths at which similar minerals are formed in the earth's crust can be determined.

Anthropometric Map of Poland

Polish scientists are completing preliminary work on the first anthropometric map of Poland. Measurements have already been taken of more than 40,000 people in various Polish cities. The data collected will make it possible to determine the prevalent average types of body structure characteristic of both sexes and of different age groups. The research is being supervised by a committee headed by Jan Mydlarski, chief of the Anthropology Institute of the Polish Academy of Sciences.

The information gathered thus far forms the basis for further measurements of 300,000 individuals. Next year teams of anthropologists will begin measuring Poland's rural population as well as its students.

AAAS-Rosenthal Cancer Prize

Lloyd W. Law, physiologist and head of the leukemia studies section of the Laboratory of Biology, National Cancer Institute, is the first recipient of the newly established AAAS-Anne Frankel Rosenthal memorial award for cancer research. The \$1000 prize, which is supported by the Richard and Hinda Rosenthal Foundation, was presented during the recent meeting of the AAAS in Atlanta, Ga.

Law's research has been concerned with the factors affecting the development of leukemia and breast tumors; the genetics of the mouse and of *Drosophila*; and the chemotherapy of neoplasms. The award committee felt that knowledge of leukemia has been materially increased by Law's admirably conceived and carefully controlled studies.

AEC Division of International Affairs

The Atomic Energy Commission has announced the establishment of a Division of International Affairs and the appointment of John A. Hall as the division's first director. Hall has been with the AEC since 1948; he has headed the Office of International Affairs, which is being absorbed in the new division. He served as director of liaison and protocol for the U.S. Delegation to the International Conference on Peaceful Uses of Atomic Energy at Geneva last August.

Working in close cooperation with the State Department and other Government agencies, the new unit is charged with the commission's functions in connection with (i) the bilateral agreements for cooperation relating to the peaceful applications of atomic energy; (ii) the proposed International Atomic Energy Agency; (iii) other matters of interest to the AEC before the United Nations such as proposals relating to disarmament; and (iv) maintaining liaison with the State Department and foreign officials in connection with atomic energy matters.

Radioactive Pharmaceuticals

"Developments in radioactive pharmaceuticals" were discussed at the annual meeting of the AAAS by Marshall Brucer, chairman of the medical division at the Oak Ridge Institute of Nuclear Studies. At the conclusion of his talk, Brucer pointed out that during the past 10 years the use of radioiodine has increased from a few millicuries to almost 50,000 millicuries per month; further, the use of radiophosphorus has increased to almost 13,000 millicuries per month, and radiogold, which was almost unknown in 1950, is now being distributed at the rate of more than 50,000 millicuries each month.

Brucer commented that "For a drug to change within 10 years from an almost unknown item to the point where its hundred millionth millicurie approaches use is a remarkable development. One hundred million of anything is a lot, and it indicates that there already is a radioactive pharmaceutical business even though there may not be a radioactive pharmaceutical industry."

Flint Ridge Cave System

■ Exploration by members of the National Speleological Society's Flint Ridge Project has disclosed that Floyd Collins' Crystal Cave in Kentucky is actually the nucleus of a cave system larger than any

other now known. This exploration has uncovered many miles of cave passages, including connections between Crystal Cave and other nearby caves, showing that integration exists in the Flint Ridge cave system.

Passages already surveyed or explored in the system now total 32 miles, making it the largest known. It is anticipated that many more miles will be added to this figure as additional known and yet-to-be-discovered passages are studied. The second largest cave is Hollich in Switzerland.

Since early 1954, systematic exploration has been conducted in Flint Ridge by the project members to compile complete data covering the cave system configuration, underground drainage, and animal life. The work was described at the recent meeting of the AAAS by Roger W. Brucker, David B. Jones, William T. Austin, and Brother G. Nicholas.

News Briefs

■ Radio waves come from at least 1936 heavenly sources, most of which are not identified with any visible object, according to a report to the Royal Astronomical Society by Martin Ryle of the Cavendish Laboratory, Cambridge, England. About 500 sources have been accurately plotted. Some 30 are large, several may be galaxies in collision, and a few are the expanding remnants of supernovas.

■ The volcano Bezmyanny in Kamchatka, U.S.S.R., considered extinct for hundreds of years, erupted on 20 Oct. Director Zladovets of the volcanological laboratory of the Soviet Academy of Sciences said on the Moscow Radio that the cloud of ashes above the crater had swelled to 6 miles in height by the middle of November.

■ A planetarium capable of projecting the motion of 8600 stars and planets was opened on 4 Dec. in Poland's heavy industry city of Stalinogrod. The planetarium, the first in the country, is named after Nicholas Copernicus.

Scientists in the News

WILLIAM F. GIAUQUE, Nobel laureate and professor of chemistry at the University of California, Berkeley, is to receive the Gilbert N. Lewis medal of the California Section of the American Chemical Society. The gold medal is awarded from time to time in recognition of special achievements in theoretical chemistry. This is the third time the medal has been given. It will be presented

to Giauque on 15 Feb. at a special award ceremony.

Giauque was selected especially for his low-temperature work, which also won him the Nobel prize. His method of using a strong magnetic field made it possible to achieve temperatures a few thousandths of a degree above absolute zero.

W. C. NIXON of the Cavendish Laboratory, Cambridge University, Cambridge, England, has arrived at the University of Redlands for a period of 3 months. The National Science Foundation has provided funds to support Nixon's stay as a visiting research associate in x-ray microscopy. He is one of the pioneers in this field.

Nixon will deliver a series of evening lectures on 7, 15, 22, and 25 Feb. During his visit he also will give informal talks and will review the progress of x-ray microscopy research at the university. Information may be obtained from Prof. Albert V. Bacz, Physics Department, University of Redlands, Redlands, Calif. Nixon will spend April, May, and June at Stanford University under the same NSF grant.

EDGAR L. PIRET, professor of chemical engineering at the University of Minnesota, has received the William H. Walker award of the American Institute of Chemical Engineers.

ROBERT L. PIGFORD, chairman of the department of chemical engineering at the University of Delaware, was awarded the institute's Professional Progress award.

DAVID TABOR of Cambridge University, Cambridge, England, has arrived at Stanford Research Institute, where he will work for a year in the control systems laboratory on problems of surface physics. He is on leave from his post at Cambridge as assistant director of research in the Laboratory for the Physics and Chemistry of Surfaces.

WALTER G. FRANKENBURG of Lancaster, Pa., has received the Cigar Industry Annual Research award in recognition of his contributions to the development of the basic science and technology of tobacco. Specifically, acknowledgement was expressed to the award winner for his thorough investigations of the chemical processes that occur in the harvested tobacco leaf, including the conversion of nicotine into a series of other substances, and for the successful application of this new knowledge to problems of the cigar industry. The award is sponsored annually by the Cigar Manufacturers Association of America in conjunction with the Cigar Institute of America.

RUSSELL W. MUMFORD, vice president and consulting engineer for American Potash and Chemical Corporation, retired recently after 35 years of service with the company. Mumford's association with the organization began in 1920 when he was named chemical engineer in charge of research and development for the company's main plant at Trona, Calif.

During succeeding years he served as assistant manager of the Trona plant, and in 1929 he became consulting engineer. Mumford was named a vice president of the corporation in 1941 and was elected a director in 1947. He continued as vice president and consulting engineer until his retirement.

HARLOW SHAPLEY, professor of astronomy at Harvard University and former director of the Harvard College Observatory, has been elected an honorary member of the National Academy of Sciences of India.

CHARLES W. MAYO, professor of surgery at the Mayo Foundation, Rochester, Minn., recently received the 1955 award of the American Pharmaceutical Manufacturers Association. He was cited for contributions to medicine and to world understanding through work with the United Nations.

DANIEL D. CUBICCIOTTI, JR., has joined the staff of Stanford Research Institute as a senior scientist in the recently formed department of chemical physics. He was formerly supervisor of inorganic chemistry research with the Atomic International Division of North American Aviation at Downey, Calif. Cubicciotti will be engaged in fundamental studies of fused salt systems and metal-gas reactions at high temperatures, thereby opening up a new field of research at S.R.I.

OTTO STRUVE, chairman of the astronomy department at the University of California, Berkeley, has been awarded the Medaille Jules Cesar Janssen for 1955 by the Institut de France.

WILLIAM SEEMAN, former chief of the clinical psychology department at the Mayo Clinic, has assumed the newly created position of associate professor of medical psychology in the department of psychology, University of Oklahoma School of Medicine.

ARCHIE O. HALLER of the department of rural sociology at the University of Wisconsin, has been named associate professor, sociology and anthropology, at Michigan State University, effective 1 July.

ELBERT DECOURSEY, brigadier general and commandant of the Army Medical Service School at Brooke Army Medical Center, was made an honorary member of the National Society of Anatomical Pathology when he attended the sixth Venezuelan Congress of Medical Scientists at Caracas 18-26 Nov.

JOHN G. GIBSON, II, research associate in medicine at Harvard Medical School and associate in medicine, Peter Bent Brigham Hospital, has been honored by the American Association of Blood Banks for research that has extended the life of red cells in collected blood. With his associates, Gibson developed a citrate-phosphate-dextrose solution that reduces the damage suffered by red blood cells during and following blood collection.

Working with Gibson were WALTER SCHEITLIN, research fellow in medicine, Harvard Medical School and Peter Bent Brigham Hospital, and WILLIAM P. MURPHY, JR., and SEARLE REES, both of whom were formerly on the school and hospital staffs.

TRUMAN O. WOODRUFF, who has been serving as research associate in physics at the University of Illinois, has been appointed research associate in the metallurgy and ceramics department of the General Electric Research Laboratory, Schenectady, N.Y.

ALBERT I. MENDELOFF of the Washington University School of Medicine, St. Louis, has been appointed associate professor of medicine at Johns Hopkins Medical School and physician on the staff of Johns Hopkins Hospital. He also has been named clinical chief of the staff in medicine at the Sinai Hospital in Baltimore, Md.

P. M. AUSTIN BOURKE, assistant director of the Irish Meteorological Service and chairman of an international group of experts established by the World Meteorological Organization to deal with plant-disease problems, has recently started a 1-year mission in Chile under the auspices of the United Nations Technical Assistance Program. He will advise the Chilean Government on meteorological control of the potato blight, which in the last 5 years has become a serious menace in Chile.

Necrology

ALBERT R. BECHTEL, Indianapolis, Ind.; 73; emeritus professor of botany and chairman of the department from 1920 to 1950 at Wabash College; 12 Dec.

EDWIN M. BLAKE, Mt. Kisco, N.Y.; 87; mathematician; 20 Dec.

BENJAMIN B. FREUD, Chicago, Ill.; 71;

emeritus professor and first chairman of the chemistry department at Illinois Institute of Technology; 12 Dec.

C. RILEY HOUCK, Memphis, Tenn.; 39; associate professor of physiology at the University of Tennessee; expert on kidney function and hypertension; 10 Dec.

SEYMOUR KORKES, Durham, N.C.; 33; associate professor of biochemistry at the Duke University School of Medicine, Durham, N.C.; 10 Dec.

SIEGFRIED W. LANDSBERGER, New Rochelle, N.Y.; 79; chemical engineer and food preservation expert; 12 Dec.

JAMES MCELGIN, Philadelphia, Pa.; 54; chemical engineer with the E. F. Houghton, Co., Philadelphia, Pa.; 7 Dec.

Education

■ The chemistry department of Howard University has inaugurated a graduate program leading to the Ph.D. degree. This is the first department in the university to begin training at this level. The department has 6 doctoral candidates enrolled in the new program.

With the initiation of the additional graduate training, the chemistry department has made arrangements for extending its activities in biochemistry by enlisting the cooperation of the department of biochemistry of the College of Medicine. Lloyd H. Newman, head of that department, and Lawrence M. Marshall and Felix Friedberg, members of the biochemistry faculty, will participate in the new curriculum of the chemistry department by offering advanced courses in special topics of biochemistry and by supervising the research undertaken by graduate students working toward advanced degrees. Together with Victor J. Tulane, associate professor of biological chemistry in the chemistry department, the four men will constitute the biochemical division of the department of chemistry.

■ *The Superior Pupil in Junior High School Mathematics* is the name of a new Office of Education bulletin by Earl M. McWilliams and Kenneth E. Brown. To secure data for this new publication, the authors visited classrooms in 140 junior high schools from Maine to California. Schools were selected because of their educational provisions for the superior pupil. These provisions are described.

The use of class activities such as mathematics clubs, contests, various conferences, and so forth are discussed. Ways of identifying superior students are presented. The publication may be obtained for 25 cents from the Superintendent of Documents, U.S. Government Printing Office, Washington 25, D.C.

■ Lowell Technological Institute has established a new 4-year course leading to the B.S. degree in general engineering. The course will train graduates who wish to cut across the traditional lines of engineering specialization; it will be available to incoming freshmen in September. It does not require the introduction of new subjects or equipment but primarily regroups present engineering courses.

■ A nurse-midwife school, perhaps the first to be offered in a university obstetrical clinic, will be resumed at Johns Hopkins University, Baltimore, with the aid of a \$75,000 grant from the China Medical Board of New York, Inc. The object is to train as many foreign students as possible, as well as American nurses planning to serve abroad, in this field of special importance in underdeveloped regions.

The board's work in China, which was primarily support of the Peking Union Medical College, has had to be discontinued since 1950 because of the prevailing political conditions. It now concerns itself with problems of medical education in all of Southeast Asia. Grants to institutions in the United States have been made primarily to further this policy.

■ A series of television programs demonstrating a fundamental medical teaching technique, the grand-rounds tour of hospital wards with discussions of significant cases by outstanding clinicians, will be telecast from Boston, Mass., on a series of closed circuits to some 50 cities throughout the country. The series will originate at the New England Medical Center and at Tufts University School of Medicine. It is being sponsored by the Upjohn Company and is a part of the Bingham Associates Program of Postgraduate Medical Education at the New England Medical Center.

The first telecast, which will take place on the evening of 18 Jan., will give physicians who view it an opportunity to watch a group of specialists participate in the "Management of acute abdominal emergencies." This program will be devoted to abdominal problems that occur in everyday practice—for example, massive gastrointestinal hemorrhage, acute appendicitis, perforated viscus, and acute gall bladder.

Grants, Fellowships, and Awards

■ The Alfred P. Sloan Foundation, Inc., has awarded the first group of a series of grants that will be made in the future from the foundation's fund for basic research in the physical sciences. This fund, which was established earlier this year, was made possible by a personal gift of \$5 million from Mr. and Mrs. Alfred P.

Sloan, Jr. The grants just announced totaled \$235,000 and were awarded to 24 faculty members who are engaged in basic scientific research at 16 colleges and universities.

■ Ten predoctoral and four postdoctoral National Science Foundation scholarships will be available for study and research at the Duke University Marine Laboratory, Beaufort, N.C., for the summer of 1956. The predoctoral scholarships are open to graduate students from any institution; the postdoctoral scholarships are limited to applicants from the Southeast. Applications must be completed by 1 Apr. Blanks and information may be secured from Dr. I. E. Gray, Department of Zoology, Duke University, Durham, N.C.

■ Two predoctoral fellowships are offered jointly by the Geophysical Laboratories of the Carnegie Institution, Washington, D.C., and Johns Hopkins University, Baltimore, Md. The fellowships cover a 2- to 3-year period. The stipend for the first academic year is \$2400. During this year the fellowship holder is chiefly engaged in course work at the university.

Starting with the second year, he will spend most of his time at the Geophysical Laboratory working on a problem for his dissertation. The duration of this period is variable and may amount to approximately 2 years. The stipend starts at \$200 per month, plus provision for university tuition, and is increased to \$225 per month during the concluding period of tenure.

Application for the fellowship, supported by transcripts and recommendations, must be sent before 1 Mar. to Prof. Ernst Cloos, Geology Department, Johns Hopkins University, Baltimore 18, Md.

■ The Office of Naval Research Advisory Panel for Microbiology will meet next spring; proposals for ONR support of research in microbiology to be reviewed at that meeting must be submitted before 1 Mar.

■ Postdoctoral research associate appointments are being offered at Brookhaven National Laboratory that provide opportunities for research in physics, chemistry, biology, and engineering. Those phases of research are emphasized which deal with studies of the structure of the nuclei of atoms, high-energy particles, the utilization of the new nuclear techniques in basic chemical and biological problems, and in engineering studies of the utilization of nuclear energy.

A research associate may formulate and carry out his investigation either independently or in collaboration with a senior member of the laboratory staff. All usual and many special instrumental facilities are available.

Research associate appointments are made for a period of 1 year; they may be renewed for an additional year. During his term, the research associate is extended all the privileges afforded to regular members of the laboratory's scientific staff. Those interested should submit their applications by 15 Feb. to Dr. R. C. Anderson, Brookhaven National Laboratory, Upton, N.Y.

■ The Oak Ridge Institute of Nuclear Studies has recently issued a new brochure describing the Oak Ridge graduate fellowship program. The brochure explains that a graduate student in mathematics, chemistry, physics, engineering, biology, or other scientific field may, on application of his graduate dean, receive a fellowship appointment in order to carry out his thesis research at Oak Ridge National Laboratory or one of the other Atomic Energy Commission laboratories at Oak Ridge.

Although a fellowship may be awarded before a student has completed his academic work, it does not become effective until he has completed the course and language requirements and the preliminary or qualifying examinations at his university. His research is supervised by a committee appointed by his graduate dean and, on successful completion of research and the fulfillment of other university requirements, the fellow is awarded his degree by the university.

Basic stipend for doctoral candidates is \$2100 for 12 months; there is an additional annual allowance of \$300 for a wife and \$300 for one or more dependent children. Master's degree students in certain sciences may also be awarded fellowships; the basic stipend for master's candidates is \$1600 for 12 months, with an additional annual allowance of \$400 for a wife and \$300 for one or more dependent children. Initial appointments are for 1 year and are renewable. Fellows may not be engaged in any other work for remuneration during the term of their appointments. Copies of the brochure and additional information may be obtained by writing the University Relations Division, Oak Ridge Institute of Nuclear Studies, P.O. Box 117, Oak Ridge, Tenn.

Miscellaneous

■ The International Commission on Zoological Nomenclature has announced that on 30 June it will start voting on the following cases involving the possible use of its plenary powers for the purposes specified against each entry. Full details were published on 30 Dec. 1955 in the *Bulletin of Zoological Nomenclature* in parts 9 and 10 of volume 11.

1) *Bithynia* Leach, 1818 (cl. Gastropoda), validation.

2) *Osmorus* (cl. Pisces) as from Linnaeus, 1758, validation.

3) *Philipsinella* Novak, 1886 (cl. Trilobita), validation.

4) *Gempylus* Cuvier, 1829, and *serpens* Cuvier, 1829 (*Gempylus*) Cl. Pisces), validation.

5) *Seguenziceratidae* Spath, 1924 (Cl. Cephalopoda, Order Ammonoidea), suppression.

6) *Lepisma* Linn., 1758 (Cl. Insecta), gender.

7) *Pagurus* Fab., 1775, and associated family-group name (Cl. Crustacea, Order Decapoda), suppression.

In addition, an application for the use of the plenary powers in the following case was published this month in part 12 of volume 9 of the *Bulletin*: Curtis, 1837, ed. 2 of *Guide arrangement Brit. Ins.*, suppression of, for the purpose of selection of type species of genera.

Comments should be sent as soon as possible to the commission's secretary, Francis Hemming, secretary to the commission, 28, Park Village East, Regent's Park, London, N.W.1.

■ The smallest and earliest human embryo, consisting of only two cells and no more than 5 days old, has been added to the collection of the Carnegie Institution of Washington's department of embryology in Baltimore, Md. This two-celled embryo—and one of 12 cells, another of 58 cells, and a fourth of 107 cells—close the last gap in the collection. It now has human embryos from the earliest stage of development to the end of the embryonic period.

■ A list of chairmen of departments of mathematics has been compiled and is now available upon request at the headquarters office of the American Mathematical Society, 80 Waterman St., Providence, R.I. Requests may also be sent to the office of the secretary of the society, Prof. E. G. Begle, Leet Oliver Memorial Hall, Yale University, New Haven 11, Conn.

■ The first article in the January issue of *The Scientific Monthly* is "Recent developments in the detection and measurement of infrared radiation" by R. A. Smith. The other articles in this issue include "Some merits and misinterpretations of scientific method" by Paul F. Schmidt, "Survey of the Gothic Natural Area" by Herbert A. McCullough, and "A biologist looks at human nature" by Ludwig von Bertalanffy.

In the "Science on the March" section there is an account of a "New attempt to cross Antarctic" by D. G. Stratton. A report by Hilary J. Deason on the AAAS traveling high-school science libraries is contained in "Association Affairs." Nine books are reviewed in this issue.

Reports and Letters

Antagonism of 5-Hydroxytryptamine by Chlorpromazine

In the course of studies of the mechanism of the vascular injury induced in rats by a group of agents that damage mast cells and liberate histamine (ovomucoid, 48/80, dextran, and testis extract), it was discovered that 5-hydroxytryptamine as well as histamine is capable of producing hyperemia and edema when injected subcutaneously into rats (1). It was also found that 5-hydroxytryptamine or a related substance is associated with mast cells and is liberated along with histamine by the "histamine liberators" (2). It has been demonstrated that dibenamine inhibits the *in vitro* action of 5-hydroxytryptamine on the rat colon (3). We therefore tested it *in vivo* as an antagonist of the edematous response to 5-hydroxytryptamine and found it a potent inhibitor. It is known that chlorpromazine has antihistaminic activity (4) and adrenergic blocking properties similar to those of dibenamine (5). Consequently, we examined chlorpromazine for its anti-5-hydroxytryptamine properties and antihistaminic properties *in vivo* in the rat and *in vitro* on the rat colon and found it to be a potent 5-hydroxytryptamine antagonist as well as an antihistaminic (6).

Female albino rats (Sprague-Dawley) weighing 160 to 200 g were used in these experiments. Subcutaneous injections of the various agents in solution in freshly made 0.86-percent NaCl were given in the dorsal skin of the paws of rats. Saline was used for control injections. The subcutaneous injections were made by carefully inserting a 27-gauge needle between the third and fourth digits of the paws to about the midpoint of the dorsum of the paw; 0.05 ml was injected in the fore paw and 0.10 ml in the hind paw. Each side of the rat, fore and hind paw, was used to measure the action of a single edema-producing agent; two agents were thus tested simultaneously in each rat. Four or more rats were used to test each dose of an agent, and the injections were rotated from rat to rat so that the right and left sides were used an equal number of times for each agent. Evans blue, 0.5 ml of a 0.4-percent solution in 0.86-percent saline, was injected via the tail vein immediately prior to the local injection. Intense bluing of the skin is an evidence

of leakage of plasma protein because the dye is bound to the plasma protein. There was little or no bluing of saline-treated paws. The following concentrations of the agents were used: 5-hydroxytryptamine (7), 1 $\mu\text{g}/\text{ml}$; and histamine, 200 $\mu\text{g}/\text{ml}$ (each as the free base). These concentrations of the edema-producing agents gave approximately the same response as measured either by the increase in tissue water content or by the grossly evident swelling and bluing of the skin (2). The local vascular response was graded grossly from 0 to 4+, 2 hours after local injection; at this time the induced edema was still maximal and the swelling from saline injection had largely disappeared. The agreement between the gross observations and the measurements of edema by increase in tissue water, estimated by removal of the skin and drying to constant weight at 100°C for 5 days, was excellent.

Chlorpromazine (8) was tested in doses of 0.5 to 3.0 mg/kg of body weight. The drug was administered in 0.86-percent saline solution via the tail vein 15 minutes prior to the local injection of histamine or 5-hydroxytryptamine. With a dose of chlorpromazine of 0.5 mg/kg, little decrease in vascular injury was observed. At doses of 1.0 and 1.5 mg/kg, the action of both histamine and 5-hydroxytryptamine was completely abolished. The 3.0 mg/kg dose produced an observable lethargy, convulsions, and occasional death in the animals.

Chlorpromazine was also tested on the rat colon as antagonist to 5-hydroxytryptamine and acetylcholine. The method used was that of Dalglish, Toh, and Work (9). Atropine was omitted from the bath fluid. The bath volume was 16 ml. For the test, the strip was standardized to give about one-half maximal contraction to each of the stimulants. For 5-hydroxytryptamine, the quantity was 0.025 μg and for acetylcholine it was 0.05 μg . Exposure of the strip to 10 μg of chlorpromazine for 3 minutes inhibited the reaction to both stimulants by 50 per cent or more. Addition of 20 μg of chlorpromazine to the bath reduced the activity to almost nothing. Complete recovery of activity then required about 30 minutes. Essentially the same result was obtained with strips from 3 rats.

The full range of the specific pharmacological antagonisms of chlorpromazine

has not been worked out. The known activities have recently been summarized (10). They are mild histamine, acetylcholine, noradrenalin antagonism and powerful adrenalin inhibition. To this we now add the 5-hydroxytryptamine antagonism (11). This action is of considerable interest in terms of the rapidly appearing evidence of the importance of this agent as a neurohumor (12). Chlorpromazine has been used with good effect in a variety of conditions including mental disease (13), nausea and vomiting (14), and alleviation of pain (15). We suggest that the demonstrated antagonism of chlorpromazine to 5-hydroxytryptamine may contribute to an understanding of the diverse actions of the drug and that the antagonism merits further investigation.

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7. 5-Hydroxytryptamine creatinine sulfate obtained courtesy of George Berryman, Abbott Laboratories, North Chicago, Ill.
8. Thorazine, Smith, Kline and French Laboratories, Philadelphia, Pa.
9. C. E. Dalglish, C. G. Toh, T. S. Work, *J. Physiol. London* 120, 296 (1953).
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1 August 1955

Method for Distinguishing Intact Cells from Free Nuclei

In recent communications, Brown (1) and Allfrey and Mirsky (2) have stressed the lack of any convenient and reliable method for distinguishing whole cells from free nuclei, particularly in the case of small lymphocytes with very scanty cytoplasm. We wish to draw attention to a simple method that has given promising results with many types of material that we have examined. This is based on

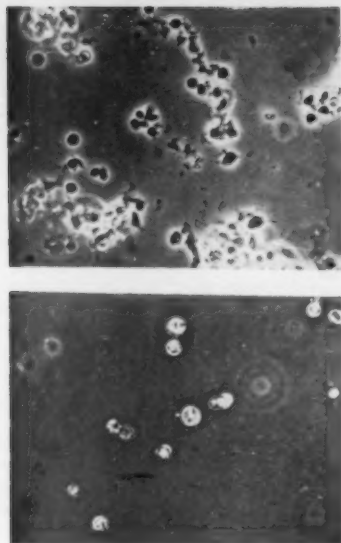


Fig. 1. (Top) Field showing free nuclei of mouse spleen immersed in a 28-percent protein solution; (bottom) field showing intact cells in the same medium ($\times 300$).

a method of cell refractometry that has been described elsewhere (3).

When living cells are immersed in an isotonic protein medium having a refractive index greater than that of the cytoplasm, the latter appears bright instead of dark by positive phase contrast. This depends on the impermeability of normal cytoplasm to proteins. If the cell is killed by fixation or other drastic chemical methods, the cytoplasm becomes permeable to proteins and remains dark. Mild physical damage or autolytic changes result in cytoplasmic swelling and bleb formation but do not usually affect the impermeability to proteins; as the refractive index decreases with swelling, the bright appearance is enhanced. Free nuclei, on the other hand, appear to be permeable to proteins and always remain dark even in highly concentrated protein solutions. The cytoplasm of most normal tissue cells has a refractive index of less than 1.370, which corresponds to a protein concentration of about 20 percent. In order to avoid excessive dilution of protein by the suspension medium it is convenient to make up a stock solution containing about 30 percent protein. Armour's bovine plasma albumin fraction V has been used for this purpose.

The adjustment of tonicity is important (3), but for mammalian tissues it is sufficient to make up a 30-percent protein solution in 0.6-percent sodium chloride. A small droplet of homogenate suspension is mixed on a slide with a large drop of protein and then examined by phase-contrast or interference microscopy. Figures 1 and 2 illustrate the tech-

nique as applied to preparations from the spleens of 7-day old mice.

The method used to prepare free nuclei (4) depends on the use of a medium containing 0.12M KCl with small amounts of NaCl and CaCl_2 and 6 percent bovine plasma albumin, brought to pH 7.1. A glass homogenizer of the Potter-Elvehjem type was used to fragment the tissue. Several methods involving the use of sucrose have also been tried, but they appear to give less satisfactory results and smaller yields.

In Fig. 1, the top photograph is a low-power view showing a field containing free nuclei in a 28-percent protein medium, and the bottom photograph shows a similar field containing intact cells.

Figures 2 and 3 show a number of higher-power views taken of a less vigorously treated preparation containing a mixture of free nuclei and intact cells. On the left in Fig. 2 is a single intact cell and a group of three free nuclei with a little granular debris. The nuclei are surrounded by the well known phase-contrast halo, but there is no bright zone of cytoplasm. The bright cells are actually surrounded by a dark halo that is more easily seen under the microscope than in the photographs. A small lymphocyte and a free nucleus are shown on the right in Fig. 2. The very narrow zone of bright cytoplasm is clearly visible under the microscope and cannot be confused with a naked nucleus. Figure 3 illustrates another interesting point: the intact cells

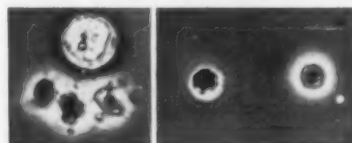


Fig. 2. (Left) Intact cell and three isolated nuclei; (right) small lymphocyte with very narrow ring of bright cytoplasm and naked nucleus. Both photographs taken in 28-percent protein medium ($\times 700$).

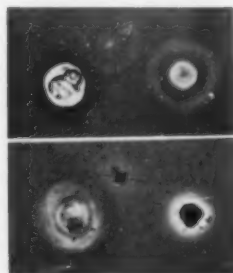


Fig. 3. (Top) Intact cell in focus, nucleus out of focus; (bottom) nucleus in focus, intact cell out of focus. These photographs illustrate the fact that cells and nuclei usually float at different levels. Both photographs taken in 28-percent protein medium ($\times 700$).

frequently float at a slightly different level from the free nuclei. In the top photograph, an intact cell has been focused and the free nucleus appears out of focus.

In the bottom photograph, the position has been reversed and the free nucleus is in focus. A tiny bleb of bright cytoplasm can be seen adhering to the nucleus. Such blebs usually disappear in the course of time.

We believe that this simple test may prove to be of value to other workers in this field (5).

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4. A detailed report of the method is in preparation.
5. This work was supported by the Rockefeller Foundation and by the Medical Research Council of Great Britain.

1 August 1955

Leap Year and Calendar Reform

John J. Case has called attention [*Science* 122, 648 (7 Oct. 1955)] to a kind of calendar reform that has been largely overlooked by calendar reformers, the returning of the zero point in the calendar year to the solstice, an appropriate point in the seasonal year. He suggests that this could be accomplished by dropping out 10 days, doing again what Pope Gregory did in 1582. The same good end could be achieved by a much less drastic operation; we have all the apparatus we need in our control of the leap year rule. Let us simply agree to do without leap years until the calendar rights itself—that is, no more this century after 1956. Then the year A.D. 2000 would be a very propitious time to begin the use of leap years again.

The adoption of this proposal would not disturb our accepted 7-day week, which is the point of greatest resistance to most proposals of calendar reform. It would even simplify slightly comparative business records for some time to come. And if, within a generation or so, we do venture a major calendar reform involving weeks and months, we should be able to get the new one smoothly running for a while without the added complication of an extra leap year day outside the weekly sequence.

When we resume leap years in A.D. 2000, we would do well to substitute a

better leap year rule for our present Gregorian rule, which is neither simple nor as accurate as it should be. Our present rule makes the average year 365.97/400 days. The actual tropical year is shorter than this, and it is growing steadily shorter by a long-term slow change. The most accurate simple leap year rule just now would be the following: a leap year every 4th year unless the year number is divisible by 128 (this gives an average year that was a perfect fit in 1910). To keep in step for thousands of years ahead, we would prefer a simpler rule giving us a leap year every fourth year unless the year number is divisible by 120. With this rule, there would be no appreciable drift over the next 8000 years. Indeed, the maximum drift up to the year A.D. 10,000 would be scarcely more than the range of drift that is inescapable within any 4-year cycle according to any kind whatever of leap year rule. By comparison, the continued use of our Gregorian leap year rule would shift the calendar more than a week by A.D. 10,000. Besides, the improved rule—every 4th year a leap year unless the year number is divisible by 120—would be easier to remember and easier to apply. Having once gotten our calendar into step with the astronomical tropical year, this rule would keep us in step for a very long time to come.

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7 November 1955

Effect of Ergot Drugs on *Betta splendens*

It has been reported in a previous communication (1) that the Siamese fighting fish, *Betta splendens*, responds to low concentrations of *d*-lysergic acid diethylamide (LSD-25) with a quiescent state that is typified by at least nine easily observable changes in the vegetative, motor, and behavioral characteristics of the fish. This communication summarizes an attempt to determine which, if any, of these nine changes are, indeed, specific for this drug (2).

The method employed consisted of exposing groups of three fish to equimolecular solutions of LSD-25 and of eight other ergot derivatives that included two optical isomers of LSD-25 (*l*-LSD-25 and *d*-isoLSD-25), a monobromo derivative of LSD-25 (BOL-148), *d*-lysergic acid, *d*-lysergic acid ethylamide (LAE-32), ergotamine, dihydroergotamine, and ergonovine. Mescaline and meperidine hydrochloride (Demerol) were also tested. The fish were observed continuously over a period of 4 hours; after this they were washed, transferred to fresh spring water, and observed at longer intervals.

Table 1. Response of *Betta splendens* to ergot drugs, mescaline, and Demerol.

Response	LSD-25	<i>l</i> -LSD-25	<i>d</i> -IsoLSD-25	BOL-148	LAE-32	<i>d</i> -Lysergic acid	Ergonovine	Ergotamine	Dihydroergotamine	Mescaline	Demerol
Backward movement with pectoral fins	x			x		x		Atypical, usually at bottom			
Head up at surface	x										x
Cartesian diver (vertical)	x			Rare		x					
Barrel-roll (vertical)	x			Rare		x					
Body kinking	x			Rare		x					
Quiescent state	x			x							
Slow deliberate movements	x			x		x		x			
Lateral display	x	x	x	x		x	x	x			x
Darkened pigment	x			x		x			x		

Experiments were performed at concentrations varying from $5 \times 10^{-7}M$ (approximately 0.2 μg of LSD-25 per milliliter) to $5 \times 10^{-6}M$ for the ergot drugs. LSD-25 was active over the complete range. Mescaline and Demerol were completely inactive at this level and were run at concentrations of 2.5 mg/ml and 0.6 mg/ml, respectively, the highest levels of these drugs that are not rapidly lethal. Table 1 reports the results of an experiment at the $5 \times 10^{-6}M$ level. It can be seen that, even at relatively high concentrations, the first five criteria—which seem to define a syndrome of loss of control of the musculature of the trunk that is possibly accompanied by a derangement of hydrostatic bladder function and the quiescent state—are sufficient to differentiate LSD-25 from the other drugs in the table. Both LAE-32 and BOL-148 resemble LSD-25 relatively closely, but both rarely induce the spastic kinking produced by LSD-25 that causes the fish to look like commas when they are viewed from the side and often like letter *s*'s when they are viewed from above. Neither do LAE-32 and BOL-148 cause the fish to assume an almost vertical position near the surface of the water for long periods of time. The symptoms induced by LAE-32 appear later than those induced by either BOL-148 or LSD-25. Fish exposed to BOL-148 in concentrations above 5 μg /ml often die; concentrations of the order of 0.5 μg /ml show no effect beside an increase in pigmentation and a decrease in activity. Since the induction of the torpor varies inversely with the dosage and may take an hour to develop at dosages of the order of 1 μg /ml, there is a possibility of mistaking the BOL-148 reaction for a response to LSD-25 at this concentration if the fish are not observed long enough.

As previously stated, the fish may become essentially quiescent for days. Arousal occurs at the slightest stimulus but is followed by an immediate return to relative inactivity. During the period of entrance into the quiescent state and during the period of emergence therefrom, the

fish have been observed to show their typical rage reaction to other fish. The rage reaction lasts only a few seconds, but the full expansion of dorsal, ventral, and (in the case of LSD-25) pelvic fins frequently occurs even though the fish are otherwise essentially quiescent.

The specific effect on *B. splendens* of the diethyl amide group of LSD-25 is therefore vulnerable to changes in either spatial or chemical variations in the structure of LSD-25. Similarly, in man only LSD-25 shows its special effects (3). However, the action on brain metabolism as measured by oxygen consumption does not depend on this spatial specificity but on the chemical structure (4).

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3 August 1955

Formation of Radioactive Protein-Bound Monoiodotyrosine by Stored Thyroid Slices

During the course of our investigations of thyroidal iodine metabolism, the observation was made that cattle thyroid slices or lobes that have been stored in the refrigerator (or deep-freeze unit) for 24 hours or more retain much of their ability to form protein-bound I^{131} from iodide- I^{131} present in the Krebs-Ringer bicarbonate buffer incubation medium. We have now investigated the I^{131} metabolism of stored thyroid slices (1) in some

detail and find that, in contrast with surviving (fresh) thyroid slices (2), stored thyroid loses ability to (i) form diiodotyrosine, (ii) synthesize thyroxine, and (iii) concentrate iodide from the incubation medium. Stored thyroid slices, however, do retain an ability to form protein-bound monoiodotyrosine and are considerably more productive in this respect than the copper-fortified thyroid homogenates that have recently been shown to be capable of producing monoiodotyrosine (3, 4).

The afore-mentioned deficiency of stored thyroid slices with respect to the ability to concentrate inorganic I^{131} was demonstrated when stored slices were incubated in media containing thiouracil, sodium *p*-aminosalicylate (PAS), or Tapazole—all of which inhibit organic binding of iodine in the thyroid. The data in Table 1 show that stored slices accumulated much less inorganic I^{131} than fresh thyroid slices. Incubations were carried out in quadruplicate for 3 hours by the procedure of Morton and Chaikoff (2); standard deviations are tabulated. The measurements summarized in Table 2 further indicate that stored slices lose ability to concentrate inorganic I^{131} . Over a 13-fold variation in the ratio of the weight of the slices to the weight of the medium and slices, the ratio of the inorganic I^{131} in the slices to that in the medium and slices changed correspondingly; and the quantity of inorganic I^{131} in the slices after incubation was never more than would diffuse into an equal weight of inert aqueous material.

The composition of the protein-bound I^{131} was established by paper chromatography, with collidine- H_2O-NH_3 development (5), of stored slices hydrolyzed (after incubation in the I^{131} medium) with pancreatin (5). Forty microliters of the pancreatic hydrolyzate were placed along a 4 cm line (origin) on Whatman No. 1 filter paper. The autoradiographs of Fig. 1 show that little if any diiodotyrosine (DIT) or thyroxine (TX) was formed and that the predominant component of the protein-bound I^{131} was monoiodotyrosine (MIT). A high R_f component similar to that found in thyroid homogenate (4, 5) was also present in the incubated, stored slices.

Formation of MIT by stored slices is apparently enzyme-dependent since a 1-minute boiling of the slices before incubation decreased MIT production by a factor of 30, and adding $10^{-4}M$ Cu^{++} to the medium did not restore MIT production (Table 3). The addition of 10^{-3} or $10^{-4}M$ Cu^{++} or Co^{++} to the incubation medium did not markedly alter MIT formation in stored, unboiled slices. Thiocyanate, at $10^{-3}M$, inhibited organic binding of I^{131} by stored thyroid. Incubation at $25^\circ C$ in air (ordinarily, incubation of thyroid slices is carried out at $37.5^\circ C$ in

an atmosphere of 95 percent oxygen and 5 percent carbon dioxide) did not decrease MIT formation, although reducing incubation temperature to $4^\circ C$ did diminish the rate of MIT production. Incubation in the presence of $10^{-3}M$ KCN or in a nitrogen atmosphere did not depress MIT formation. Homogenizing stored slices before incubation reduced protein-bound I^{131} production by a factor of 6—to about the level found in studies with thyroid homogenates (3-5).

From the time of the identification of monoiodotyrosine as a natural constituent of the thyroid gland (6), MIT was thought to represent the first step in the formation of DIT (7). The results of this study show, however, that a relatively large proportion of MIT can be produced with the formation of only minimal quantities of DIT. Further, the observations that MIT is produced when incubation takes place in the presence of

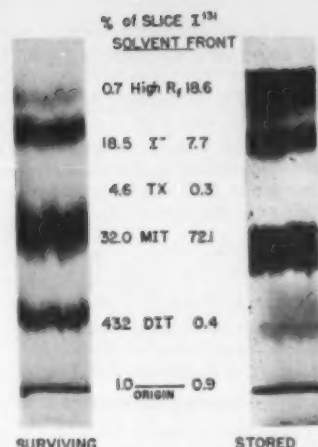


Fig. 1. Autoradiographs of chromatograms from surviving and stored thyroid slices incubated in I^{131} medium.

Table 1. Comparison of surviving and stored thyroid slices.*

Compound	I^{131} in slices, as percentage of total I^{131} in medium plus slices			
	Surviving		Stored	
	Protein-bound I^{131}	Inorganic I^{131}	Protein-bound I^{131}	Inorganic I^{131}
Controls	51.1 ± 3.8	12.1 ± 1.0	36.9 ± 3.0	4.1 ± 0.3
$10^{-4}M$ thiouracil	2.8 ± 0.2	21.6 ± 2.0	0.8 ± 0.1	3.5 ± 0.2
$10^{-4}M$ thiouracil	6.2 ± 0.4	19.7 ± 1.4	1.0 ± 0.1	4.4 ± 0.4
$10^{-4}M$ PAS	3.2 ± 0.1	19.9 ± 1.6	1.5 ± 0.1	3.6 ± 0.1
$10^{-4}M$ PAS	9.6 ± 0.3	25.8 ± 1.1	3.2 ± 0.1	4.5 ± 0.4
$10^{-4}M$ Tapazole	5.0 ± 0.4	20.3 ± 0.9	1.1 ± 0.1	3.8 ± 0.3

* All stored thyroid slices employed in the measurements for Tables 1, 2 and 3 were stored at $-16^\circ C$ for 2 weeks before incubation.

Table 2. Effect of varying ratio of slice weight to medium volume.

Slice weight (mg)	Medium volume (ml)	wt. of slices/ wt. of medium and slices	I^{131} of slices/ I^{131} of medium and slices
150 mg slices,	3.0 ml medium	1/21	1/24 \pm 2
150 mg slices,	12.0 ml medium	1/81	1/99 \pm 8
600 mg slices,	3.0 ml medium	1/6	1/7.3 \pm 0.6

Table 3. Factors affecting stored thyroid metabolism.

Factor	I^{131} in slices, as percentage of total I^{131} in medium plus slices		
	Total I^{131} in slices	Protein-bound I^{131}	Inorganic I^{131}
Controls	41.1 ± 2.9	36.9 ± 3.0	4.1 ± 0.3
Boiled 1 min	4.0 ± 0.1	1.1 ± 0.1	2.8 ± 0.1
Boiled + $10^{-4}M$ Cu^{++}	4.2 ± 0.2	1.3 ± 0.1	2.8 ± 0.1
$10^{-4}M$ Cu^{++}	40.6 ± 2.5	36.8 ± 2.7	3.9 ± 0.3
$10^{-4}M$ KSCN	5.3 ± 0.4	3.2 ± 0.3	2.1 ± 0.3
$10^{-4}M$ KCN	40.2 ± 3.7	35.0 ± 3.1	4.3 ± 0.3
$25^\circ C$ in Air	42.6 ± 3.8	38.4 ± 3.2	4.1 ± 0.4
N_2 Atmosphere	41.9 ± 3.2	37.2 ± 2.8	4.2 ± 0.4
$4^\circ C$	8.2 ± 0.7	6.1 ± 0.5	2.3 ± 0.2
Homogenized	7.0 ± 0.5	6.3 ± 0.4	0.7 ± 0.1

CN⁻ or in an atmosphere of N₂ or air suggest that the formation of protein-bound MIT is not strongly dependent on the oxidative, energy-producing cycles of the cell. This is in contrast with the apparent dependence on those cycles of the formation of DIT and the concentration of iodide in the thyroid (8).

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1 August 1955

Protection of Mouse Fetus against X-irradiation Death

The value of cysteamine (beta-mercaptoethylamine) as a protective agent against ionizing radiations in the adult mammal was first established by Bacq (1) and has been confirmed by other investigators. In our laboratory, a study by Rugh and Wang (2) indicated that the effect of 700-r x-rays, the minimum LD_{100/30} for CF₁ adult male mice, is modified by the intraperitoneal injection of 3 mg of cysteamine 5 to 30 minutes before irradiation so that only 30-percent mortality occurs within the specified period. It was thought to be important to determine whether the protective value of this -SH compound could be transferred to the mammalian fetus that is x-irradiated *in utero* by the injection of the drug into the pregnant animal. Although the radiosensitivity of the fetus has been extensively investigated in terms of developmental abnormalities and lethality (3), no such protection studies on the fetus have been reported in the literature.

The animals used in this study (4) were CF₁ female mice that were x-irradiated on gestation days 13.5 to 19.5. Doses ranged from 300 to 1200 r, delivered at a rate of 96.5 r/min air dose as

measured at the level of the gravid uterus. The x-ray facilities consisted of a Quadrocondex constant-potential therapy machine run at 210 kvp peak, 15 ma, and a distance of 50 cm from the target to the animal, with 0.28 mm Cu and 0.50 mm Al filters added. Cysteamine was made up in physiological saline at a concentration of 3 mg/ml, 1 ml of which was administered intraperitoneally to the pregnant mouse 20 minutes before exposure to x-rays.

Each experiment consisted of three groups of mice. One control group received cysteamine without irradiation. A second control series received x-irradiation alone; the third or experimental group of mice received both the drug and x-irradiation. After delivery of the offspring, the litters were counted daily and weighed at weekly intervals for 1 month. In cases in which the mice received a lethal dose of irradiation, litters were exchanged at birth with simultaneously dropped control litters in order that death or possible retardation of lactation in the irradiated mothers might not affect the growth and viability of the offspring. Data from cysteamine-injected controls have in no way differed from uninjected control values.

Considering the weight at age 1 month, although the irradiated mice show an average weight difference of almost 5 g less than that of the non-irradiated controls, prior administration of the drug allows average weight very nearly equal to that of the controls (Table 1).

In Table 2 appear data from an experiment in which an exchange was

made between treated litters and mothers and noninjected controls. Both irradiated groups showed a decreased birth weight below control weights with but slight difference between the two treated groups. All fetuses irradiated without cysteamine died within the first 10 days after delivery, while those that had received prior cysteamine injections exhibited 78.6 percent survival (protection to 1 month).

It seems reasonable to hypothesize that a reduction in litter size, particularly in the noncysteamine-injected but irradiated groups, could be the result of their greater susceptibility to death *in utero*. In the data obtained following irradiation at 14.5 days of gestation (Table 1), the percentage values might seem to suggest a range of sensitivity favoring the cysteamine-injected mice. However, the control and drug-injected groups are quite similar in weight, and an apparently significant weight difference exists between these two groups and those that received 300 r alone. The birth weight differences in the 700-r, 17.5-day series (Table 2) are those between the controls and both the irradiated groups. In terms of 1-month survival, however, it seems obvious that the survival of any mice in the cysteamine-treated group provides evidence for the drug's protective effect, for none of the 66 controls survived.

As can be seen from the data on survival and weight to 1 month after delivery, cysteamine provides some protection against x-irradiation not only for the adult mouse, but also for the fetal mouse. This protection is expressed as a weight

Table 1. Data for mice irradiated with 300-r x-rays at 14.5 days of gestation. The percentage survival to age 1 month is based on the number alive at birth.

Treatment	Av. litter	Natal mortality (%)	Av. birth wt. (g)	Survival to 1 mo (%)	Av. wt. at 1 mo (g)
Cysteamine alone (14 litters, 136 mice)	9.7	2.2	1.70	76.0	11.74
X-rays alone (6 litters, 50 mice)	8.3	14.0	1.18	40.0	6.83
Cysteamine and x-rays (6 litters, 55 mice)	9.1	14.5	1.37	54.5	11.44

Table 2. Data for mice irradiated with 700-r x-rays at 17.5 days of gestation. The percentage survival to age 1 month is based on the number alive at birth.

Treatment	Av. litter	Natal mortality (%)	Av. birth wt. (g)	Survival to 1 mo (%)	Av. wt. at 1 mo (g)
Cysteamine alone (14 litters, 136 mice)	9.7	2.2	1.70	76.0	11.47
X-rays alone (13 litters, 66 mice)	5.1	3.0	1.25	0	
Cysteamine and x-rays (12 litters, 92 mice)	7.7	8.7	1.30	78.6*	8.60

* Data for three litters.

difference in mice that received 300 r at 14.5 days of gestation and as a reduction of the lethality following 700-r x-rays at 17.5 days' gestation. It is postulated that related -SH compounds with demonstrable protective value in adults might lead to a similar conclusion if they are applied to the fetus.

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4. This paper is based on work performed under contract AT-30-1-GEN-70 for the U.S. Atomic Energy Commission.
5. Cysteinamine was obtained from Harvey Blank of the E. R. Squibb Company and was known as Becapant hydrochloride.

22 July 1955

Serum Cholesterol in Men in Basal and Nonbasal States

In comparative studies of serum cholesterol concentrations in population samples (1), it is difficult to assure that the conditions under which blood is drawn are exactly the same in different investigations. But what difference does it actually make if bloods are not drawn under basal, fasting conditions?

Over a period of 2 months, we drew blood on 4 to 7 mornings from each of 16 men on the staff of this laboratory (2). The basal fasting state was represented by several blood samples from each man; the other samples were drawn in mid-morning when the men could be interrupted from their customary laboratory and desk work. The nonbasal serum in this series averaged 3.8 mg of cholesterol per 100 ml higher than the amount in the basal serum (3). This difference amounted to 1.9 percent of the mean basal value.

Another comparison was made with samples from male students of the University of Minnesota, 100 of whom were studied in the basal, fasting state while 300 others came in between classes several hours after a normal breakfast. On the average, the values for the nonbasal men were 3 percent higher than the values for the basal men.

Results of a more systematic study on clinically healthy men are summarized in Table 1. Cholesterol was measured in duplicate in serum from each man in the basal, fasting state and again 2 hours

after a breakfast consisting of fruit, cereal with cream and sugar, two slices of toast with butter and jam, and coffee with cream and sugar. In one series of experiments (series A), 10 g of pure cholesterol were emulsified in scrambled eggs, and these "fortified" eggs were substituted for the cereal; thus the total cholesterol intake in the meal averaged 10.8 g.

In both series A and B, in which the men were relatively inactive after breakfast, the serum total cholesterol tended to rise. The mean increase at 2 hours was 2.41 percent of the basal value in series A and 1.83 percent of the basal value in series B. These changes are highly significant statistically. However, in series C, when breakfast was followed by moderately vigorous physical work (walking on the treadmill or working in the garden), the postbreakfast rise did not occur; instead, the mean value tended to fall from the basal level. The difference between the exercise and nonexercise responses is highly significant, the mean difference being 8.28 mg percent with a standard error of ± 2.25 mg percent.

To examine the effect of exercise on the response to cholesterol added to the meal, 3 experiments were performed on each of 4 healthy young men. Blood was sampled in the basal state and at 2, 3, 4 and 7 hours after breakfast in each case. In series D, the meal was the aforementioned ordinary breakfast, but in the other two sets of experiments, 10 g of cholesterol were added to scrambled eggs, which were substituted for the cereal. The subjects were sedentary in series D and E, but in series F they walked on the treadmill for 45 out of every 60 minutes. Under all conditions in the experiments of series D, E, and F, the serum cholesterol concentration tended to be higher after the meal than before; it was highest when the subjects were sedentary after they had received the 10-g cholesterol load. The data are summarized in Table 2.

The cause of the rise after breakfast when sedentary conditions were maintained cannot be ascribed to the cholesterol in the meal. In series A and D, the total increase of cholesterol in the blood was greater than the total cholesterol in the meal. The explanation for these phenomena would seem to illustrate the essential role of cholesterol in fat transport. In the sedentary state, the serum cholesterol rise must reflect a rise in lipoproteins, meeting the demand for transport of the newly absorbed fats. However, when there is physical activity during this absorption period, the enhanced rate of circulation, plus nutrient withdrawal from the blood to meet the increased metabolic needs, results in a lower serum

concentration of lipoproteins and hence of cholesterol.

Series	N	Mean (Δ)	S.D. (Δ)	S.E. (Δ)
A	51	4.92	± 11.72	± 1.66
B	73	3.95	± 11.74	± 1.37
C	15	-3.93	± 12.95	± 3.34

Table 2. Average increases of serum total cholesterol (mg/100 ml) in 4 men after breakfast. In series D and E, the men were sedentary, while in series F they were physically active. In series E and F the breakfast contained 10 g of added cholesterol.

Time (hr)	Series D	Series E	Series F
2	4.2	9.3	7.3
3	3.2	11.5	7.8
4	3.0	13.5	4.3
7	3.7	15.0	9.3

concentration of lipoproteins and hence of cholesterol.

Clearly, the difference in cholesterol concentration between basal and nonbasal blood drawn in the morning is so small that it may be neglected in most comparative studies, particularly if the nonbasal subjects are engaged in physical work. In addition to settling this point, the results cited suggest a reason for part of the difference in susceptibility to coronary heart disease that is reported in comparisons between active and inactive men (4).

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5 August 1955

Scientific Meetings

Antibiotics in Agriculture

The First International Conference on the Use of Antibiotics in Agriculture was held in the U.S. Department of Agriculture Auditorium in Washington, D.C., 19-21 Oct. 1955. It was sponsored by the NAS-NRC Agricultural Research Institute and was supported by funds contributed by the American Cyanamid Company, Merck & Company, Chas. Pfizer & Company, Inc., and E. R. Squibb & Sons. More than 400 persons attended the conference, from the United States and at least 15 other countries.

The conference was called for the critical evaluation of present knowledge with respect to growth and other responses of livestock to low-level antibiotic feeding, antibiotic use in crop plant production and food preservation, the influence of antibiotics on antibiotic-sensitive microflora, antibiotic effects on reproduction and carcass quality in livestock, antibiotic residues in human food, the mode of action of antibiotics, and the delineation of problems requiring research.

Young animals of our livestock species commonly respond to continuous oral administration in their feed of about 10 parts per million of one of several antibiotics by increased gain in weight about 10 percent greater than controls, under farm conditions. The growth response is generally less or absent in young of the same species under optimal conditions of nutrition and sanitation, although Luckey (University of Missouri) reported a few data, for chicks and turkeys, that he concluded indicated growth response in a germ-free environment. Rats respond only when their diet is inadequate; guinea pigs, when fed chlortetracycline, may develop *Listeriosis* and die. Data for children who were fed antibiotics continuously for 2 years, reported by Squibb (Servicio Cooperativo Interamericano de Agricultura, Guatemala) showed no unfavorable responses and no important net growth response, although some groups apparently did show an initial growth response.

Clausen (Denmark) demonstrated that low-level antibiotic feeding had no deleterious effect on carcass quality of swine when the antibiotic-fed swine were

restricted to the same feed intake as the controls. He warned, however, that since antibiotic feeding generally increases feed intake, pigs that are fed *ad libitum* on low-protein diets might, therefore, become excessively fat. Some U.S. research workers have reported that protein level in the diet of antibiotic-fed pigs may be decreased. Clausen's report was, therefore, timely, and swine nutritionists must accept or refute his findings.

No effect of antibiotic feeding on reproduction of livestock has been demonstrated, but neonatal mortality is reduced in antibiotic-fed chicks, poults, pigs, calves, and lambs. The therapeutic use of antibiotics for treatment of livestock diseases is selectively effective. Both Pomeroy (University of Minnesota) and Finland (Thorndike Memorial Laboratory, Boston) noted that disease-producing strains have not been found to emerge among livestock that are raised for market on antibiotic-supplemented feeds. This is in contrast to experience with human beings and, as already noted, with guinea pigs.

The emergence of tolerant or resistant pathogens requires additional intensive research. Finland reported that *Micrococcus aureus* has been most troublesome in man; the incidence of strains of *M. aureus* that are resistant to any antibiotic appears to be related to the frequency and intensity with which that antibiotic has been used. J. J. Christensen (Minnesota) presented a beautifully illustrated report on variations, including many mutations, that occur in fungi grown in media that contain antibiotics. Apparently, no mutants have yet been reported in soil microflora as a result of antibiotics produced in the soil under natural conditions. The ecology of antibiotic-producing fungi was discussed by Mishustin and Krasilnikov (U.S.S.R.). Mishustin reported important effects of antibiotics in inhibiting nitrogen fixation by *Azotobacter*. Krasilnikov reported crop plant protection against disease by absorption of antibiotics produced by fungi naturally occurring in the soil. Application of antibiotics to crop plants for control of many diseases was reported by Zaumeyer (U.S. Department of Agriculture). A principal problem that remains to be solved is that

of finding antibiotics that are effective in concentrations low enough to be economical for crop use.

Zaumeyer reported that antibiotics are absorbed by the plant. It has been a question to what extent they were absorbed by animals at the 10 parts per million level that is characteristic of feed use. Gordon and his colleague Taylor (England) presented assays and clearly demonstrated systemic presence of antibiotics when the antibiotics were fed at these low levels. A challenge method for evaluating the prophylactic hypothesis of mode of action of low levels of antibiotics, which was also proposed by Gordon, will be useful in the further research that is needed to establish mode of action.

Most of the speakers suggested or conceded that some effect of antibiotics on some of the intestinal microflora must be a principal factor in low-level antibiotic feeding. The general absence of nonspecific enteritis in young antibiotic-fed animals, the general lack of growth response of chicks in new quarters, and some direct research, reported by Combs (Maryland), on effects on intestinal flora support but by no means demonstrate the validity of the prophylactic hypothesis. Eagle (National Institutes of Health) succinctly pointed out that it remains to be demonstrated which microbial species are sensitive to 10 parts per million levels of which antibiotics.

Baumann (University of Wisconsin) reported evidence that, in the rat, antibiotic feeding stimulated vitamin synthesis in the intestine, and that in the chick, the thinner intestinal wall that is characteristic of antibiotic-fed chicks may be more permeable to nutrients. Freerksen (Germany) supported the antimicrobial prophylactic hypothesis, especially with respect to organisms abnormally present in the upper gut. However, he stated that the prophylactic hypothesis is insufficient and reasoned that antibiotic feeding must result in supplying directly essential substances, perhaps hormone precursors. François (France) reported evidence that antibiotics depress deaminase activity in the intestine of the pig. Johansson (Minnesota) pointed out that the effects of antibiotics on mammalian cells in tissue culture must be determined and suggested that the Metchnikoff hypothesis of auto-intoxication be reexamined. Other evidence was presented that suggests an effect of antibiotics on thyroid activity. It was generally agreed that determination of mode of action of antibiotics at low levels must be made in order to make further research on their use fully effective.

Tarr (Canada), Deatherage (Ohio), and others reported convincing evidence that low levels of antibiotics added post-

Kodak reports to laboratories on:

how to bring somebody else's slide lecture back home... riding the new speed vs. sharpness curve... adding to an already imperfect list of reagents

Convention trophies

Bumped into a lad the other day who is director of photo services at one of the big Middle Western universities. It was at a meeting of a technical society, and he had spied us in the act of outwitting a speaker who was impressing the audience by zipping slide after slide of big, fat tables of numerical data onto the screen. There we sat in the dark with a little camera, popping away at each slide so that when we got home we could take our own sweet time deciding just how impressed to be.

We thought we would have a chance to expound to the fellow a little on film, aperture, and shutter speed involved in this photographic feat, but it turned out this type of cagey operation is straight routine with him and we couldn't tell him a thing he didn't know already. One of the regular duties of his office is to supply staff members with cameras to catch the slides at meetings.

With any luck and a steady hand, a camera no more elaborate than the Kodak Pony 135, Model C, which comes with an $f/3.5$ lens for \$33.75, should pick up readable copies of slides on Kodak Tri-X Film when shot wide open at 1/25 sec. from a point in the room where the screen image just fills the finder. It could provide something more definite to bring home from a meeting than a feeling of solidarity with one's discipline. (Caution: copying a man's slides without his permission could conceivably earn one a punch in the nose.)

Enlarging the breakthrough

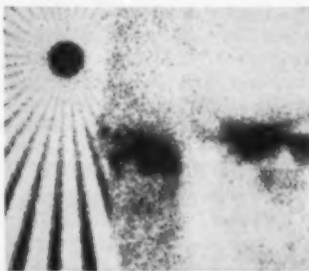
Looked back at now, the film emulsion-making practices of, say 1953, seem unsophisticated. We shall have to let the remark stand at that. The fact is that there has been an abrupt rise in a quasi-quantitative quantity, the product of film speed and sharpness.

About measuring speed there is a lot to be said, but we won't say it here, except that the *Kodak Tri-X Film* which we introduced a year or so ago has significantly expanded the scope of photography.

Sharpness, a subjective impression, has now likewise had a metric imposed on it. There is a mathematical statement—termed acutance—of the density variation across the

photographic image of a knife edge. It is quite different from resolving power, a quantity related to the smallest repetitive detail distinguishably reproducible, whether the detail looks sharp or not.

Riding the new speed vs. sharpness curve, we here announce reactivation of the name *Kodak Panatomic-X Film* to apply now to our sharpest roll film, 35mm and other sizes, for general photography. Its Exposure Indexes are 25 for daylight and 20 for tungsten. Its emulsion is less than half as thick as usual in negative film. This shortening of the path along which light can scatter on its way down is part of the reason for the greater sharpness, but only part. The thinness also speeds processing. As for resolving power, witness the following demonstration, as filtered through the press that printed this page:



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Diethyldithiocarbamic Acid Sodium Salt (Eastman 2596) for example, forms interfering colors with iron, cobalt, nickel, and bismuth and calls for solicitude over monochromatism in applying Beer's Law to measure the yellow complex it forms with copper. *Diphenylthiocarbazone* (Eastman 3092) demands close watch on pH to keep a reasonable specificity, besides being confusingly colored itself. *2,2'-Bi-quinoline* (Eastman 6183) has the Cu specificity, but with a molar absorptivity index of only 5900, it is a little low on sensitivity.

Compare this figure with the 16,000 molar absorptivity index of the cupric complex of dicyclohexanone oxalylidihydrazone, which one Scandinavian chemist first prepared by reacting oxalyl dihydrazide with cyclohexanone and two other Scandinavian chemists put to work measuring copper in paper pulp. The sensitivity works out to about 0.03 ppm. Of 48 other common ions tested, not one gave an interfering color. Lead, zinc, nickel, and particularly cyanide did interfere, but forthrightly, by preventing or delaying color formation, not by dissemblance.

Beyond a doubt, even this negative interference will someday exasperate some analytical chemist into devising yet another reagent for copper. We hope to sell that one, too.

Will you accept a gift, meanwhile, of a procedural abstract for dicyclohexanone oxalylidihydrazone in the determination of copper in serum? If you also want the chemical, we're afraid we have to charge \$2.50 for 5 grams of it as Eastman 7175. Distillation Products Industries, Eastman Organic Chemicals Department, Rochester 3, N. Y. (Division of Eastman Kodak Company).

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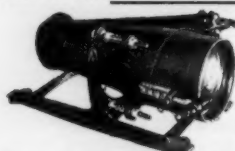
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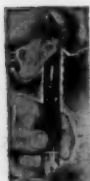
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mentally to fish, chicken, and meat have value in preserving these foods. Such meat, when processed under sanitary conditions, is preserved for several days without refrigeration; chilled, unfrozen fish and chickens are preserved for several days longer than is now possible. These and other speakers emphasized the necessity for establishing means of assuring the absence of antibiotic residues in foods as eaten. Randall (Food and Drug Administration) reviewed occurrence of antibiotics in food and reported very low levels of penicillin in a substantial percentage of milk samples, probably as a result of penicillin treatment of cows for mastitis. He pointed out the possible danger, for the occasional exquisitely sensitive human being, of small amounts of antibiotics in food, and reviewed the provisions of the Food, Drug and Cosmetic Act that are relevant to the use of antibiotics in food production. Rigorous tests for safety will properly delay use of antibiotics in food preservation until the public can be legally and biologically assured that there is no health hazard.

The conference was climaxed by the scholarly address of Rene J. Dubos (Rockefeller Institute of Medical Research), the discoverer of gramicidin, who reviewed basic research on microbial physiology. The proceedings of the conference will be published by NAS-NRC and will be available about April 1956.

T. C. BYERLY

Agricultural Research Service,
U.S. Department of Agriculture,
Washington, D.C.

Meeting Notes

The following scientists from abroad have accepted the invitation of the Tissue Culture Association to participate in an International Decennial Review Conference to be held at Woodstock, Vt., 8-12 Oct. 1956: H. E. Street, University of Swansea, Wales; Roger J. Gautheret, Sorbonne, Paris; E. N. Willmer, Cambridge University, Cambridge, England; W. H. Schopfer, University of Bern, Bern, Switzerland; Jacques Monod and George Barski, Pasteur Institute, Paris; George Morel, National Agronomic Institute, Versailles, France; Peter J. Gaillard, University of Leyden, Leyden, Netherlands; Etienne Wolff, College de France, Paris; Honor B. Fell and Ilse Lasnitzki, Strangeways Laboratory, Cambridge, England; A. Moscona, Hebrew University, Jerusalem, Israel; O. A. Trowell, British Atomic Laboratories, Harwell, England; Maurice Chevrement, University of Liège, Liège, Belgium; Edith Paterson, Christie Cancer Hospital, Manchester, England; George Klein, Karolinska Institute, Stockholm, Sweden; Charles Lumsden, Maida Vale Hospital, London, England; Wilhelmina de Bruyn,

National Cancer Institute, Amsterdam, Netherlands.

Institutions in the United States or Canada that may wish to take advantage of the presence of these persons in this country as lecturers are urged to communicate with them, with the organizing secretary of the conference, Dr. Philip R. White, Jackson Memorial Laboratory, Bar Harbor, Maine, and with Dr. Paul Weiss, National Academy of Sciences, Washington, D.C., (chairman of the Biology Year Coordinating Committee). It is hoped that coordination of this sort may serve to make the stay of these scientists more interesting and useful and to spread the expense of their voyage.

The 1956 Western Joint Computer Conference and Exhibit is to be held at the Fairmont Hotel in San Francisco, 7-9 Feb. This conference is one of the two national conferences on electronic computers that are sponsored jointly each year by the American Institute of Electrical Engineers, the Association for Computing Machinery, and the Institute of Radio Engineers. For information write to the chairman of the publicity committee, Donald C. Holmes, Shell Development Company, Emeryville, Calif.

A Washington, D.C., chapter of the Association of Technical Writers and Editors, as authorized by the national meeting of this new society, has been formally organized, with a membership that will probably start with more than 50 persons in Government and private employment in the Washington area. Temporary officers of the chapter are: chairman, E. M. Cohn, U.S. Bureau of Mines; v. chairman, W. J. Miller, National Research Council; sec.-treas., R. T. Hall, U.S. Forest Service; programs, W. N. Ezekiel, U.S. Bureau of Mines; by-laws, R. H. Schaaf, Department of Defense, and H. B. Simpson, Westinghouse Air Brake Co.; and publicity, N. J. Pritchard, Operations Research Office.

The first regular program meeting of the chapter will take place on the evening of 18 Jan., at George Washington University (Building C). The program will consist of a forum on the general topic, "Technical writing—through the consumer's eye" and will include 15-minute talks from three different viewpoints.

Forthcoming Events

February

5-8. National Citizens' Planning Conf., Washington, D.C. (Miss H. James, 901 Union Trust Bldg., Washington 5.)

9-10. Soc. of American Military Engineers, annual, Chicago, Ill. (D. A. Sullivan, 72 W. Adams St., Chicago 90.)

13-17. American Soc. of Civil Engineers, Dallas, Tex. (ASCE, 33 W. 39 St., New York 18.)

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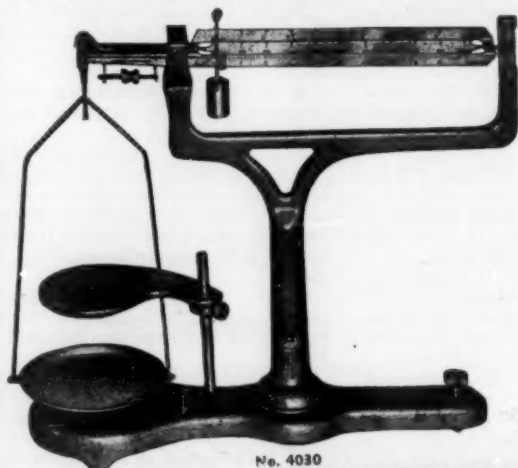
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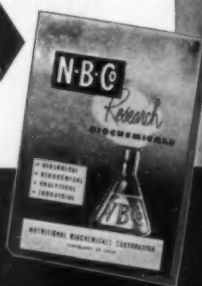
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16-17. National Conf. on Transistor Circuits, 3rd, Philadelphia, Pa. (J. D. Chapline, Remington Rand, Inc., 2300 W. Allegheny Ave., Philadelphia 29.)

19-23. American Inst. of Mining and Metallurgical Engineers, New York, N.Y. (E. O. Kirkendall, AIME, 29 W. 39 St., New York 18.)

19-23. Soc. of Economic Geologists, New York, N.Y. (O. N. Rove, Union Carbide and Carbon Corp., New York 17.)

20-22. American Educational Research Assoc., annual, Atlantic City, N.J. (F. W. Hubbard, AERA, 1201 16 St., NW, Washington 6.)

23-25. National Soc. of College Teachers of Education, Chicago, Ill. (C. A. Eggertsen, School of Education, Univ. of Michigan, Ann Arbor.)

24-25. American Physical Soc. Houston, Tex. (K. K. Darrow, APS, Columbia Univ., New York 27.)

26-29. American Inst. of Chemical Engineers, Los Angeles, Calif. (F. J. Van Antwerpen, AIChE, 25 W. 45 St., New York 36.)

28-29. Scintillation Counter Symposium, 5th, Washington, D.C. (G. A. Morton, RCA Laboratories, Princeton, N.J.)

March

12-16. National Assoc. of Corrosion Engineers, 12th annual, New York, N.Y. (Secretary, NACE, Southern Standard Bldg., Houston 2, Tex.)

14-17. National Science Teachers As-

soc., Washington, D.C. (R. H. Carleton, NSTA, 1201 16 St., NW, Washington 6.)

15-16. Food Physics Symposium, 1st international, San Antonio, Tex. (C. W. Smith, Southwest Research Inst., San Antonio.)

15-17. American Orthopsychiatric Assoc., 33rd annual, New York, N.Y. (M. F. Langer, AOA, 1790 Broadway, New York 19.)

15-17. American Physical Soc., Pittsburgh, Pa. (K. K. Darrow, APS, Columbia Univ., New York 27.)

15-17. Kappa Delta Pi, annual, Stillwater, Okla. (E. I. F. Williams, 238 E. Perry St., Tiffin, Ohio.)

16-18. International Assoc. for Dental Research, St. Louis, Mo. (D. Y. Burrill, 129 E. Broadway, Louisville 2, Ky.)

18-24. American Soc. of Photogrammetry, annual, joint meeting with American Cong. on Surveying and Mapping, Washington, D.C. (ACSM-ASP, Box 470, Washington 4.)

19-22. American Acad. of General Practice Scientific Assembly, 8th annual, Washington, D.C. (AAGP, Broadway at 34th, Kansas City 11, Mo.)

19-22. Inst. of Radio Engineers National Convention, New York. (E. K. Gammett, IRE, 1 E. 79 St., New York 21.)

19-23. American Soc. of Tool Engineers, Chicago, Ill. (H. C. Miller, Armour Research Foundation, 35 W. 33 St., Chicago 16.)

21-22. National Health Forum, New

York, N.Y. (T. G. Klumpp, National Health Council, 1790 Broadway, New York 19.)

21-23. American Power Conf., 18th annual, Chicago, Ill. (R. A. Budenholzer, Illinois Inst. of Technology, Chicago 16.)

21-24. American Astronomical Soc., Columbus, Ohio. (J. A. Hynek, McMillin Observatory, Ohio State Univ., Columbus 10.)

23-24. Eastern Psychological Assoc., Atlantic City, N.J. (G. G. Lane, Univ. of Delaware, Newark.)

24-25. American Psychosomatic Soc., 13th annual, Boston, Mass. (T. Lidz, APS, 551 Madison Ave., New York 22.)

24-31. Perspectives in Marine Biology, La Jolla, Calif. (A. A. Buzzati-Traverso, Scripps Institution of Oceanography, La Jolla.)

25-28. American Assoc. of Dental Schools, annual, St. Louis, Mo. (M. W. McCrea, 42 S. Greene St., Baltimore 1, Md.)

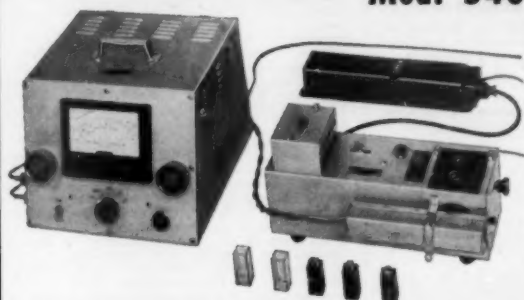
25-29. American College Personnel Assoc., Washington, D.C. (Miss C. M. Northrup, Univ. of Denver, Denver, Colo.)

28-3. Colloquium on Frontiers in Physical Optics, Boston, Mass. (S. S. Ballard, Visibility Laboratory, Scripps Institution of Oceanography, San Diego 52, Calif.)

29-31. Pennsylvania Acad. of Science, Indiana. (K. Dearolf, Public Museum and Art Gallery, Reading, Pa.)

(See 16 Dec. issue for comprehensive list)

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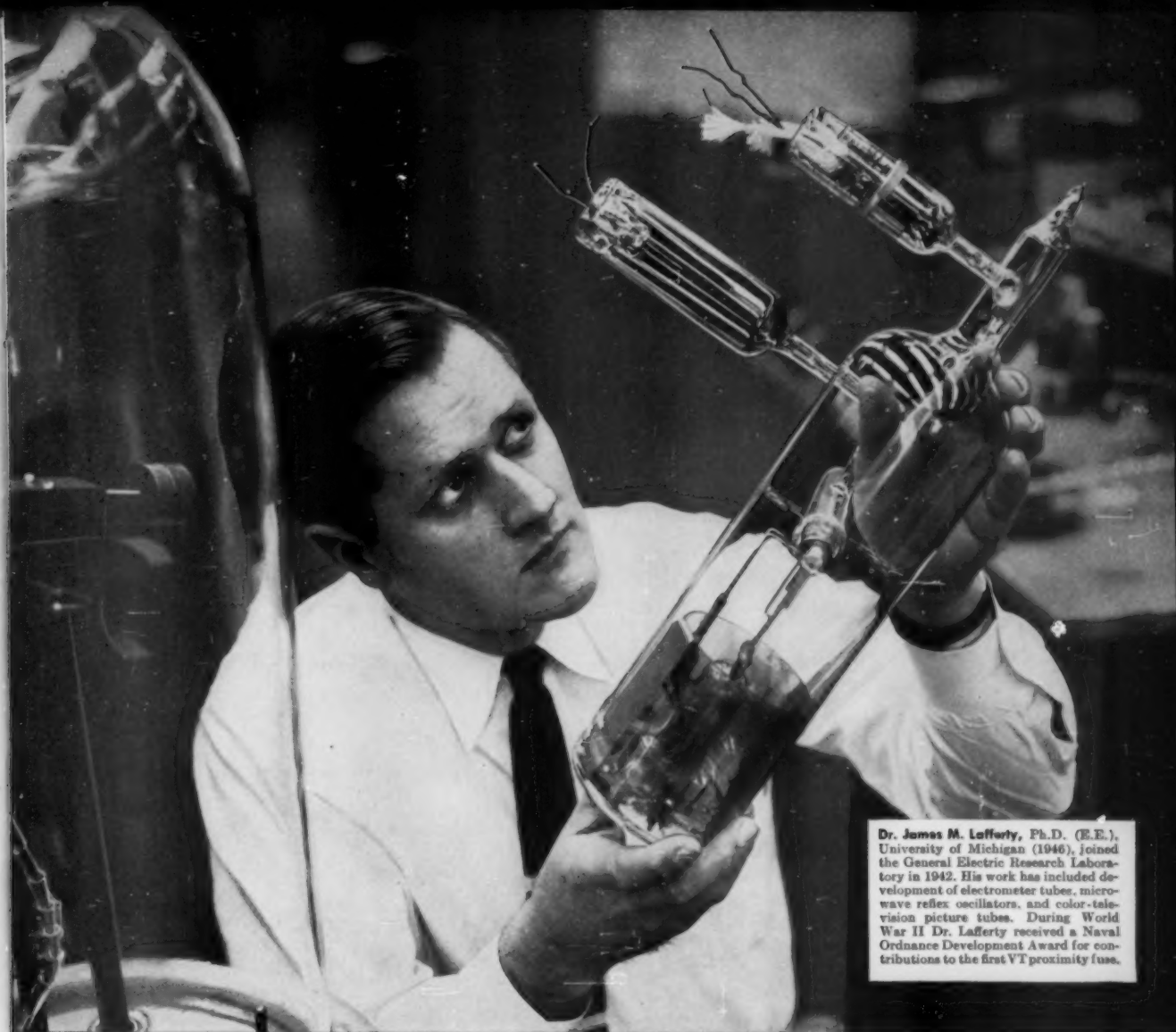
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Dr. James M. Lafferty, Ph.D. (E.E.), University of Michigan (1946), joined the General Electric Research Laboratory in 1942. His work has included development of electrometer tubes, microwave reflex oscillators, and color-television picture tubes. During World War II Dr. Lafferty received a Naval Ordnance Development Award for contributions to the first VT proximity fuse.

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